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THE

VIRGINIA

MEDICAL JOURNAL:

COMBINING THE

STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL.

Editors:

JAMES B. McCAW, M. D.—GEORGE A. OTIS, M. D.

"Without the aid of system, the informations of experience must slide imperceptibly down the tide of oblivion"—MACLURG.

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VIRGINIA MEDICAL JOURNAL.

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COMBINED.

JANUARY 1856.

ART. I.—*On the Operation of Hysterotomy.* By WILLIAM A. PATTESON, M. D. Richmond, Virginia.

IN the course of the present year, the two following cases have occurred in my obstetrical practice, both of which are characterized by such rare organic maladies of the uterus as to entitle them to be recorded, as evidence of the necessity and safety of the practice pursued.

On the morning of the 15th May 1855, I was called to visit Mrs. **** in labor with her twelfth child. I had frequently attended this lady in her antecedent confinements, and therefore had no anxiety about her, since I knew her to be strong and healthy, of full size and well developed pelvis, and that her accouchements were easy of performance and safe. I found her in active labor, and on making the primary digital examination, to my very great surprise, was unable to find any os uteri. I most carefully searched in every direction, and in various postures of the body, for it, until I became satisfied it was not to be found, but that the uterus had undergone some organic change, and that the os had suffered entire occlusion. I so advised her husband, and

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also of what I considered the proper professional treatment, to wit, an incision of the uterus in the vagina, at the point where the os should have been found. It was demanded, from the activity of the labor, since rupture of the walls of the uterus somewhere would have followed, or death, if an artificial opening was too long delayed. I desired a consultation and the presence and assistance of a professional friend. In the course of a few hours, Dr. F. B. Deane of Richmond visited her with me, and after an equally patient investigation, concurred fully in opinion as to the obliteration of the os uteri, and the necessity and propriety of incising the womb where the os ought to have been found. I should here remark, that just at this spot, on the round ball of the uterus which filled the upper strait, there was imparted to the finger a slight bulbous feel, in idea like the smooth ball which is often seen on a pane of glass. I knew here should have been the os tincæ, but it was as dense, strong, tough and unyielding to pressure as any other part of the protruding womb, and would not give way to forcible pressure from the finger. It was firm, organized, plastic growth, perfectly obliterating the os, and conglutinating its lips.

We then proceeded to the operation, and the patient was placed precisely in the position necessary for the application of the forceps. Using the index finger of the left hand as a director to the spot where I supposed the os to have been, I passed a scalpel, with a long handle held in the right hand, down to it, keeping the side of the blade in close apposition with the finger, so as to avoid wounding the vagina. For the purpose of making the opening sufficiently large, I ran the point of the knife farther down towards the recto-vaginal junction, and then turning its cutting edge to the womb, made a bold cut through its walls, drawing the knife forward, at the same time towards the symphysis pubis, directly through the supposed spot of the obliterated os uteri. The waters gushed out, accompanied and rather followed by several ounces of blood. I then passed my finger into the

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uterus through the cut, and swept it around the child's head, so as to give a circular form rapidly to the wound, which speedily occurred. The scanty bleeding soon ceased, the parturient throes went on normally, and in a few hours this lady was safely delivered of a full sized, healthy female child. The secundines were thrown off by the healthy, natural contraction of the womb, and her recovery was just as speedy and safe as if nothing untoward had occurred. The scalp of the child was cut for about an inch in length, which at its birth was covered over by a dense, smooth coagulum of blood, closely adhering to it, suspending all hæmorrhage. A broad strap of adhesive plaster was put on the wound, and not removed for ten days, when it had thoroughly healed by first intention.

During the convalescence of this patient, after the first week, the finger was gently and carefully passed through the os tincæ daily; and at a later period, every day or two, a large sized metallic bougie, warmed and oiled, was as carefully passed through the os and cervix into the womb, to keep them open, and prevent union by cicatrization. This was continued some days after the suspension of all lochial discharge. Both mother and child are now in perfect health.

In connection with this report, it is proper to remark that this lady, about two years before this accouchement, and of course about fifteen months before her last conception, had been the subject of severe uterine disease. There was at that time active inflammation and ulceration of the os tincæ. She was entirely cured by the use of leeches, rest in bed for several months, and cauterization with nitrate of silver, and Churchill's solution of iodine.

This is the second case of entire occlusion of the os uteri which I have ever witnessed. The first occurred in this city many years since, and terminated fatally, from the operation being, as I now think, too long delayed. Death happened in that case from gangrene, most likely the result of long continued and useless pressure before the operation. The

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woman was soon delivered after the section of the womb, and the case promised to do well, but gangrene followed, and she died, I think, on the second day after the operation.

The safety of the procedure of vaginal sections of the womb to effect delivery, if not too long delayed, are now sufficiently tested, and when such cases occur, it should be promptly executed, as the best practice for both mother and child. In confirmation of this opinion, the professional reader is referred to Ashwell on Diseases of Woman, chapter on Occlusion and Rigidity of the Cervix Uteri.

II. The other case, to which allusion was made in the beginning of this paper, is one of extreme rigidity of the cervix and os uteri, absolutely precluding delivery without a vaginal section of the uterus. It is curious in its history, final development and successful issue. In January 1855, I was asked to visit Amy, a servant girl of Mr. H**** of this city, who complained of prolapsus uteri. The examination revealed a singular state of things. The cervix uteri was found protruding beyond the vulva, soft, elongated, but little sensitive, patulous, so that the finger could pass easily its whole length into the cervix, which would fold along back before the finger, like that of a soft glove, and pass up into the fundus of the vagina, where you found the body of the uterus. It seemed to be prolapsus only of a soft elongated cervix. I directed her to be kept steadily in bed, and passed up a light charred wood pessary, which kept the cervix well up, whilst she lay still in bed. She was not sick, and being restive and intractable, would remove her pessary, which she said gave pain. Others were tried with no better success, until I passed her a gilt globe pessary, which she wore rather better, but not patiently. At this time I had no suspicion of this girl being pregnant; but after a few weeks attention, I found the uterus enlarging, seeming to contain some solid body, and lying permanently on the left of the mesial line, and in the left iliac fossa. A patient examination now made, and in company with a medical friend of experience, failed to satisfy us

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whether she was pregnant, or that some fibrous growth was forming in the womb. She persistently denied any pregnancy. She was kept as quiet as possible, much indulged, would sometimes wear the globe pessary, and often go weeks without it, obstinately. I made her an occasional visit, and some time in the month of March became satisfied she was pregnant, and directed her to be regarded and treated accordingly. There was now no prolapsus of the cervix. It seemed to be puckered up, and she made no complaint of anything but slight tenderness of the belly.

On Sunday, 8th July 1855, I was summoned in haste to visit this girl in labor, and was informed by the midwife, quite a sensible one, on my arrival, that there was a prolapsus of the womb. The girl was in active labor. On a digital examination, I found the os tincæ just within the vulva, and dilated to about the size of a shilling piece, the head of the child low down in the lower strait, the pains very strong, and on every effort it seemed as if the whole uterus would pass out of the vulva before the body of the child, and no dilatation of the cervix and os going on. The finger passed through the os into the womb, disclosed the cervix and parts all around it to be hypertrophied, thickened, very hard; and it was most obvious that no impression was made on the os and cervix by the parturient throes, and that delivery never could happen in this state of things, without a tearing off of the os and cervix from the body of the womb. This girl had now been in labor from the morning before, and no sort of progress had been made in dilatation for many hours. I at once decided to make the vaginal Cæsarean section, and asked a consultation. In a short time she was visited by my friend Dr. Haxall, who, after a careful examination, fully concurred in my opinion of its safety and necessity.

Passing the index finger of the left hand into the uterus as a director, I carried a blunt pointed bistoury on it sufficiently far down, and cut through the thickened cervix and os for about one and a half inches posteriorly towards the

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recto-vaginal junction, and then forwards with another cut towards the symphysis pubis of about the same length. The substance cut had a hard and gristly feel. The labor pains were very active and strong, and in about a half hour she was delivered of a feeble child, which lived only four days. It might have reached the eighth month, but was poor and weak. In a short time she was relieved of the secundines; no untoward symptom followed; she speedily recovered; is now perfectly well, and makes an excellent wet nurse. In the same manner as in the preceding case, the os and cervix were prevented by passing the finger occasionally through them from any abnormal cicatrization, until the lochia had ceased to flow.

It is fortunate for humanity that such cases, in proportion to the whole number of births, are rare. Still, two such examples, the one of closure, and the other of rigidity, where incision was successfully practiced, have fallen under my own observation in a short period of time, and serve to strengthen and fortify the practice of early vaginal hysterotomy in all such cases.

ART. II.—*Practical Treatise or Strictures of the Urethra.* By J. F. REYBARD, M. D. Translated from the French. By GEO. C. STARKE, M. D. Hicksford, Virginia.

Stricture of the Urethra.

Definition.—Stricture is a temporary or permanent diminution of the calibre of the urethra in one or more points.

The morbid changes of the urethral walls in the constricted parts have different causes, and are also of different natures: their common effect is to interfere with the emission of urine, or render this act impossible.

There are four kinds of stricture generally admitted:

1. Spasmodic stricture.
2. Inflammatory stricture.

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3. Organic stricture.

4. Stricture from deviation of the canal by development of intra or extra-urethral tumors.

In this treatise organic stricture will be studied almost exclusively. Its history, pathogeny, nature, diagnosis and treatment will be given.

Organic Stricture.

We understand by organic stricture, the permanent and progressive diminution of the diameter of the urethra, produced by gradual retraction of a pathological tissue, which is substituted for a portion of the urethral walls; its most immediate effect being to oppose the passage of urine, and offer an obstacle more and more difficult to overcome.

This defines clearly the urethral lesion which occupies us. Organic stricture, thus defined with its characteristic features, cannot be confounded with spasmodic and inflammatory stricture, nor with certain obstructions of the canal, more or less complete, caused by the development of tumors.

Spasmodic coarctations result from a temporary modification of the contractility of muscular element which enters into the composition of the canal, and has no connection with alteration of tissue. They are lesions of function, not durable, not accompanied by any change of the organic texture of the urethra.

Inflammatory coarctations, caused by a serous, sanguineous or purulent infiltration of the urethral tunics, do not oppose greatly the passage of urine. The difficulty diminishes as the inflammation subsides, and as the exuded liquids are removed. These strictures cannot come under the head of organic stricture, according to our definition.

The definition excludes also the obstructions and obliterations of the canal produced by the presence of intra or extra-urethral tumors.

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Causes.

In the etiology of organic stricture, we recognize predisposing and occasional causes.

Predisposing.

Sex.—Man is almost exclusively the subject of stricture. The urethra being long, curved, narrow and complicated in its structure, is more liable to morbid alterations than the female urethra, which is almost rectilinear, short, large and of simple organization. It is extremely rare to find a stricture in the female urethra, but I mention it because the sex is not absolutely exempt.

Age.—Stricture is rare in infancy. Old persons are seldom the subjects of a recently formed stricture, middle age being the period when strictures are most commonly formed.

Residence.—Strictures are more common in large cities and in warm climates.

Temperament.—More frequently met with in persons of nervous temperament, somewhat plethoric, and addicted to excesses.

Occasional Causes.

The occasional causes of stricture have a powerful and immediate influence in the production of this affection, which as yet have not been well appreciated, notwithstanding the numerous monographs on the subject.

We think it better not to pursue the course heretofore followed, but at once to fix our attention on the efficient causes of organic stricture, to analyze the constant phenomena which attend the formation of the morbid tissue, and take up the difficult task of explaining its nature.

Although the occasional causes are numerous and various, they may be placed in two categories. In the first, we class

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wounds of the urethra, contusions and solutions of continuity—for instance, wounds by cutting instruments, or rents from violent torsion of the penis while erect—injuries produced by straightening the penis in chordee, (*casser la chorde,*) or injuries from ligatures—injuries produced by sounds, lithotrity instruments and foreign bodies arrested in the canal—finally, lesions from blows, or falls on the perinæum. Ulcerations should be classed under the same head, whether they are developed spontaneously, or depend on a gangrenous state from excess of inflammation arising from compression or cauterization, or from loss of the urethral walls, as by excision of strictures.

Causes of the second category are those which develop or maintain inflammation of the urethra, as blennorrhagia, simple or virulent. The origin of the urethral inflammation is itself very various. It may be from gonorrhœal pus, pus of chancre, abuse of coitus, masturbation, prolonged erections, riding on horseback, catheterism, presence of foreign bodies, irritating injections, immoderate use of beer, cantharides, &c. &c. It is also said that some vices of constitution, as scrofula, were capable of maintaining the inflammation when once developed.

Every one knows that the causes above cited are those which give rise to stricture, but according to our idea, these causes have not been well studied. Some persons even believe that the different causes produced particular kinds of stricture. It is necessary to correct such errors and prevent false notions about the treatment of stricture.

Causes of the first category.

We have studied for a long time the etiology of traumatic stricture, and we divide solutions of continuity into those which produce constrictions and those which do not. We have resorted to experiments to learn the action of solutions of continuity in the urethral walls in the production of stricture, and have arranged them into three groups.

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1. Wounds and ulcerations spontaneously developed; or succeeding inflammation, gangrene, loss of substance by cauterization, excision, &c.

2. Wounds made by cutting instruments, rents by violent torsion of the penis when erect, &c.

3. Contusions, and solutions of continuity resulting from blows, or falls upon the perinæum.

First group—ulcerations.

Spontaneous ulcerations, and wounds of the urethra which cause a loss of substance, are not generally considered capable of forming strictures. This is the opinion of those in favor of cauterization—an opinion very erroneous, as we shall see.

Those who admit that these lesions will give rise to stricture do not appear to know their action, or the mechanism in the formation of constrictions. They suppose that stricture is produced by the retraction of the inodular tissue only. Our own impressions are very different.

Our conclusions are derived from experiments on animals. And although the penis of the dog is not in all respects like the human, this fact will not invalidate the correctness of our conclusions.

On the 6th November 1842, three dogs of medium size were cauterized. The canal of each admitted a sound six millimetres in diameter. To introduce the caustic I used a small canula with a rod which fitted exactly—the caustic placed in the canula, and the extremity of the canula which was to enter the canal, filled with a soft piece of wax, pointed. The canula was introduced in the urethra to the depth of five centimetres, and with the rod the caustic was forced out of the canula into the urethra.

First experiment with five centigrammes of nit. argent. No tumefaction of the penis—no hœmorrhage, no difficulty in urinating. At the expiration of five days there was a slight purulent discharge, which persisted until the wound cica-

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trized. The cicatrization was complete about the fifteenth day, for about this time the introduction of a sound did not make the canal bleed, and the purulent discharge had ceased.

Second experiment with seven centigrammes of nit. argent, deposited 5½ centimetres from the meatus. Some hours after the operation, slight hæmorrhage, which lasted 24 hours—slight tumefaction of the organ.

Third experiment with 10 centigrammes nit. argent. The cauterization was deeper in the canal, and more extensive. Hæmorrhage during two days; great difficulty in urinating; tumefaction of the penis.

The first and second dogs were kept forty-five days, to permit the cicatrice of the wound to grow hard and become organized.

The third dog died the tenth day from retention of urine. The canal was opened longitudinally, and we discovered a membranous eschar 12 to 15 millim. long, resembling a tube, entirely detached except at its extremities. There was not a *wound* under the eschar, but a red, dry, smooth surface upon the external fibrous membrane of the corpus spongiosum. On a level with this red surface the calibre of the canal was not diminished. The newly formed tissue had not obstructed the canal, and it is remarkable that there should have been retention after swelling of the penis had disappeared.

The urethra of the first dog presented a circular stricture two millimetres long, with great difficulty penetrated by a bougie two millimetres in diameter.

The canal of the second presented also a circular stricture two millimetres long, just large enough to admit a bougie one millim. in diameter. In neither case were the urethral walls at the obstruction engorged or thickened; and with the exception of constriction, they presented no appreciable alteration. The tissue of the cicatrice was thin; at least, the walls were not thicker here than elsewhere. It was easily distinguished, however, by its hardness and more compact texture, altogether inextensible, and

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we were not able to pass a bougie through the constriction, if its diameter was greater than that of the narrowed point of the canal. The retraction of the cicatrice was very manifest—exhibited not only by the stricture itself, but especially by the extent of the new tissue, when compared with the size of the wound upon which it was formed.

It appears that the eschar which succeeds cauterization is detached about the seventh or eighth day, sometimes later. The cicatrization of the wound is completed between the fifteenth and twentieth day. We presume the cicatrization is complete when the blood ceases to flow after the repeated use of dilating instruments.

According to these experiments, made by Dr. P. Brun and myself, in presence of M. Rey, professor à l'école vétérinaire, no one can doubt that ulcerations of the urethra are direct causes of stricture.

These experiments alone are sufficient to prove the uselessness and even the danger of cauterization as a method of curing stricture; but to be justified in condemning this method, it is necessary to show its insufficiency and inconvenience when combined with dilatation. This I intend doing by reporting a series of experiments, in which, after cauterization, I have endeavored to obtain much dilatation of the cicatrice. In each instance, when the dilatation was suspended for several days, the cicatrice having no obstacle to its retraction, shortened and produced stricture.

Three dogs, about the same size and strength, were cauterized as in the other experiments.

Eight days after the operation, when we supposed the eschar detached, we commenced dilatation of the canal with the largest sound which would enter. It was six millim. in diameter. At this period the canal had experienced no constriction. The sound, introduced morning and evening for the first eight or ten days, met no obstacle. But after this time the canal commenced to grow smaller, and I was obliged to replace the first sound by another, one millim. less. I continued the dilatation twenty days, when I sup-

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posed the cicatrization complete. In two cases the cure was temporary; that is, the canal was not sensibly constricted during the treatment.

To judge correctly the retractility of the cicatrix, we interrupted the dilatation in two dogs—one suddenly, the other gradually. The treatment was thus conducted three weeks; and at that time a sound four millim. in diameter could be introduced. The dilatation in neither instance was permanent, and the constriction was as great in one as the other. Two months after the operation, they had the same difficulty in urinating. By considerable efforts the urine was expelled by drops.

In the third dog, the stricture formed, notwithstanding dilatation. It progressed so rapidly, that at the end of a month it was impossible to introduce the smallest sound. The urine escaped by drops. Sometimes, if great efforts were made, it escaped in a stream about the size of silk thread.

Two months after cauterization, the animals were killed. We found the following state of things: That portion of the canal in front of the stricture was healthy—in the portion of the canal corresponding to the groove of the os pénien, the mucous membrane was of a dark red color for an extent of five millim. A narrow circular stricture, so small, particularly in the third dog, that the head of a sewing needle could not penetrate the stricture, about two millim. long, effaced the urethra as if it had been cut by a ligature.

The tissue of the cicatrix was of a bright red color, shining, and smoother than the mucous membrane. It was dense, compact, not extensible in the least, as we satisfied ourselves by the fruitless efforts to introduce sounds.

These experiments show the retractile nature of cicatrices, moreover that this property increases as the cicatrix becomes more organized, and we cannot prevent the consequent shortening, by dilating the tissue during the process of cicatrization. I have even observed that this retractile

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nature of cicatrices was increased by the use of sounds, and that stricture was produced more rapidly in cases where catheterism was practiced twice daily, than when the operation was resorted to only once in twenty-four hours. Cauterization, then, destroys a stricture by substituting another, often more extensive, more complete and more grave.

Our experiments cannot be objected to, because made on the healthy urethra instead of stricture itself.

The tissue of a cicatrix forming stricture is vital and organized, as the healthy tissue, and can it be destroyed without interfering with the healthy tissues, or at least without laying them bare? Can this tissue be taken away, and its place not supplied with another of the same nature? Can any one object to our experiments because we did not permit a sound to remain in the canal after cauterization, that the cicatrix might form upon it? Is the sound left in the human urethra after cauterization until the wound is entirely cicatrized?

Reason, clinical observation and experiments on animals, confirm our opinion on the formation of organic stricture from ulcerations produced by loss of substance of the urethral walls, and particularly by cauterization.

Second group—wounds.

We pass on to the second group of traumatic causes of stricture, that is, solution of continuity by cutting instruments. These wounds are arranged into two orders—longitudinal and transverse.

Longitudinal wounds.—Do such wounds cause stricture? Some authors believe that longitudinal wounds do produce stricture, from the fact that stricture sometimes results from the operation for stone, and also after scarifications of the mucous membrane. It is very important to refute this opinion, and it is easy to do, by reason, clinical observation and experiments on animals.

a. It is probable that wounds from scarification have

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been considered causes of stricture, because such scarifications do not cure stricture. This proposition will be amply noticed in the chapter on urétrotomy.

b. "A proof (says M. Civiale) that strictures may result from a longitudinal wound, is that we sometimes find them after the operation of lithotomy."

We are far from such an opinion. If we recollect how rare it is to see a stricture after lithotomy, we cannot think that a longitudinal division of the urethra produces stricture. In this operation the urethra is divided quite extensively, and there is no precaution to prevent the narrowing of the canal. The sound is not left constantly in the urethra, and the edges of the incision inflamed by the urine seldom unite by first intention. Not more than one case in a hundred is followed by stricture; and when it does happen, it is owing to inflammation of the wound which invades the urethral walls, and not to the incision. To admit the contrary is to make the exception a rule.

c. Every longitudinal wound of the urethra, superficial or deep, as incisions in other tissues, has two bleeding surfaces—two lips; the urethra habitually closed; the lips of the wound are contiguous, and the cicatrix from their reunion, however thick, does not diminish the diameter of the canal. The cicatrix being between the edges of the wound, might enlarge the urethra; but more commonly being linear, it can only make a difference in the length of the canal.

Notwithstanding this theoretical proof and clinical observation, which offers us the urethra after lithotomy, exempt from stricture, I have thought it necessary to experiment on animals.

d. By the aid of sounds I found the capacity of the urethra of a dog, and afterwards with the double blade urétrotomé made a deep incision. Each incision, three centim. long, embraced the entire thickness of the urethral walls. After the cicatrization of the wounds, the same sound which gave me the capacity of the canal, was introduced with the same facility. On the twentieth day, the urethra was exa-

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mined. Its walls were in a state of perfect integrity; the diameter of the canal not diminished, and a white line, two or three centim. long, marked the cicatrice.

Longitudinal wounds have no influence in the production of stricture. But it is not sufficient merely to prove the innocence of such wounds. We wish to prove, that so far from causing constrictions, they are the best means we have to increase the diameter of the canal.

Transverse wounds.

The effects of transverse wounds of the urethra is a question almost new, and I have been obliged to study them by experiments. In this order of solutions of continuity are grouped ruptures of the canal by violent torsion of the penis during erection, rents produced by sounds and lithotrity instruments, calculi engaged in the urethra, and wounds resulting from resection of strictures, or of a portion of the canal.

We have a pretty good idea of the formation of stricture consecutive to transverse wounds, by observing what passes in the canal after amputation of the penis.

The contraction of the urethra, where divided, is evident, and sometimes so very marked that there is difficulty in finding the canal. In one instance we saw Dr. Bouchet of Lyons obliged to enlarge the canal with a bistoury, and resort to the frequent use of sounds to prevent the contraction.

Dr. O. of Boulieu in 1827, while deranged, attempted suicide with a razor. One wound was inflicted on the penis, cutting the urethra and a portion of the corpora cavernosa, in front of the scrotum. When I saw the patient, hæmorrhage from the wound had ceased. My first thought was to introduce a sound to prevent infiltration of urine, and facilitate the dressing of the wound by making the parts more firm. In vain I tried the introduction of sounds of different diameters. The point of the instrument invariably

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escaped at the wound, or was engaged in the subcutaneous cellular tissue. There was much hæmorrhage, and I was not able to find the vesical end of the urethra in the wound. I placed the edges of the wound in contact, but used no plaster or compress, fearing that the dressing might obstruct the anterior portion of the urethra, and force the urine to escape by the wound.

Notwithstanding the precaution taken to arrange exactly the lips of the wound, I did not expect immediate reunion. But whether from the exact apposition of the parts, or from the work of reparation, twenty-four hours after the accident the urine was passed by the meatus, and cicatrization was prompt as if the wound had been limited to the skin. The seventh day sutures were removed, and in a short time the patient was well. The patient experienced acute pain when urinating the first time after the accident. This pain diminished, and in a few days disappeared. Neither the patient nor I suspected stricture to result from the wound. We were mistaken. Six months afterwards I examined him, and discovered the constriction. The emission of urine was impeded, and a sound of five millim. passed with difficulty.

In this case it is very certain the stricture was caused by the wound of the urethra; that is, by the inextensible cicatrice which constricted the canal, as a ligature would have done.

I divided the urethra of two dogs completely, and in less than eight days the wound was entirely cicatrized. A white, small line alone indicated the cicatrice in the corpora cavernosa.

After the operation the urine continued to pass by the meatus. Although the canal was well divided, the lips of the wound united immediately. Twelve days after the operation, when the cicatrice had acquired its ordinary consistence, an examination of the canal revealed the existence of stricture at the seat of injury.

In the first experiment the urethra admitted a sound four millimetres; in the second, the constriction was more marked, admitting only sounds less than three millimetres.

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Twenty-five days after the operation, the dogs were killed, the penis separated from the body, and a sound introduced in each extremity of the canal. Dissecting the corpora cavernosa, we found its cicatrice continuous with that of the urethra. Separating the urethra from the corpora cavernosa, and opening it longitudinally to the stricture, we had before us the orifice of the constriction. The cicatrice presented so much the appearance of an annular bridle, that one might believe it was caused by ligature of the urethra. To overcome the constriction, it was necessary to divide the cicatrice in every direction, and very deeply. The tissue of the cicatrice was white, hard and in some points cartilaginous—the mucous membrane wrinkled, with folds converging towards the cicatrice.

From the foregoing remarks, we feel justified in stating that solutions of continuity, longitudinal, are not causes of organic stricture—but transverse wounds are sure causes.

Third group—contusions.

The third and last group of traumatic causes of stricture comprises contusions and solutions of continuity, the effects of blows or falls upon the perinæum.

Contusions most commonly produce a tumor, sanguineous infiltration into the substance of the urethral tunics, and this extravasation of blood gives us two perfectly distinct phenomena: 1. Permanent organic stricture, consecutive to inflammation or suppuration of the infiltrated tissues; and 2. Stricture, the immediate obstruction developed by the bulging of the mucous membrane into the canal.

Let us remark that blows too feeble to contuse the perinæal tissues, act violently upon the canal, although deeply seated. This happens from its anatomical disposition, which we will recall.

Surrounded by soft parts, nearly the whole of the urethra escapes the effects of contusions. That portion, however, between the suspensory ligament and the ligament of Car-

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cassonne, is very much exposed. Its situation is unfavorable, being near the pubis, a solid support, against which violent blows can crush it. Its erectile tissue is composed of large communicating cells, the two sheaths, internal and external. The first is thin, soft, extensible. The second is thick, rigid and inextensible. Liquids accumulating in this areolar structure, press against the two membranes. The internal yields, and the canal is obstructed or obliterated by the projection.

The spongy tissue is more particularly injured by violence on this region. At its expense is formed the sanguineous intra-parietal tumor which mechanically obstructs the canal.

If the spongy tissue was no thicker during life than it appears on the cadaver, it would be difficult to explain why it is injured from blows, and the superficial parts not. The cells of the spongy tissue, whatever M. Franc may say, are not more friable than those of cellular tissue. On the contrary, they are more solid and resisting; but filled with blood during life, it is their state of plenitude and turgescence which causes them to be torn by external violence, while the perinæal tissues are uninjured.

At the site of the ruptured cells is formed an irregular cavity, more or less large, according to the number of cells destroyed. This cavity is filled with blood, forming a tumor limited in size by the remaining healthy cells and by the external and internal fibrous membranes. The tumor obstructs the passage in proportion as it encroaches on the canal. But this narrowing of the urethra ought not to be confounded with that produced by the compression of a tumor developed outside of the urethral walls. Both may arise from the same cause, it is true, and both oppose the passage of urine; but the difference in the situation of the tumors is important in a therapeutical point of view. No author, I believe, has drawn this distinction before. Generally, they have been contented in admitting that retention of urine consecutive to contusions of the perinæum, was due to compression exercised on the canal by a sanguineous

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tumor developed in the perinæal tissues outside of the urethral walls.

When the spongy tissue alone is injured, I regard the contusion as being of the first degree; but if one or both of the fibrous membranes be involved, it is the second degree.

Contusions of the first degree.

In the most simple form, there are only a few of the cells destroyed, and often there is no appreciable tumor; no pain; no difficulty in urinating. When the lesion is more extensive, there is acute pain at the moment the blow is received. A tumor forms on the canal, which is easily felt, although deep seated. In a short time the patient complains only of heaviness and numbness in the perinæum, with desires to urinate. In attempting to urinate he finds it difficult or impossible. The patient should rest in bed, leeches often applied over the tumor, and poulticing after the topical bleedings. The case should be closely watched, for the symptoms are not always proportionate to the extent of disorganization in the tissues. Sometimes, from very slight contusion, we see in a few days grave troubles—retention, abscess, &c. The urethra should be examined during the treatment with gum bougies, *bougies à boules*, which acquaint us with the extent of the tumor, and the obstruction produced, better than other instruments. If the difficulty of urinating increases, and with the bougie we find the tumor increasing in size, instead of resolution, suppuration is taking place, and incisions must be made into the tumor.

Contusions of the second degree.

When the urethra has received the shock, particularly, the membranes enveloping the corpus spongiosum are torn at the same time the cells are destroyed. If one or both of these membranes are ruptured, the blood escapes by the urethra; or, on the other hand, is infiltrated into the tissues

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outside of the walls of the urethra, even into the perinæal tissues forming a tumor with more or less ecchymosis. Often the bloody tumor and ecchymosis extend to the scrotum and inner part of the thighs. Contusions, which injure the cells of the corpus spongiosum and tear the membranes simultaneously, are the most common.

Of all the complications of such accidents, I think the urethral obstruction the most serious. It demands special attention to prevent or combat retention and urinary abscess, which occur so frequently. In every case of contusion the attention should be directed to the traumatic stricture. Its presence is readily detected by an examination of the canal with *bougies à boule*, and when discovered, if the retention of urine cannot be overcome by catheterism, incisions through the perinæal tissues to the tumor must be practiced. First. Raise the scrotum; make an incision along the raphé, dividing the tissues down to the canal; introduce the left fore finger into the wound to separate the edges, to be more certain of the presence and direction of the urethra and to guide the bistoury. Second. Cut the external tunic of the canal and the spongy tissue for an extent of four or five centim. according to the volume of the tumor. During this part of the operation a sound should be introduced, if possible. Third. Divide the mucous membrane; introduce a sound, which should not be taken out but once in twenty-four hours.

Incision of the external membrane and spongy tissue relieves the stricture temporarily. For permanent relief, the entire thickness of the urethral walls must be divided, as we have advised; and regular dilatation of the canal after the incision is indispensable.

Before operating, we should be perfectly convinced that bougies cannot pass the obstruction. An injection of olive oil should be thrown into the canal; conical bougies, and bougies with enlargements of the point, ought to be fairly tried.

Having spoken of the treatment of contusions, and of the

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immediate constriction of the canal, we give now the terminations of such lesions, if not interfered with.

Sometimes the bloody tumor terminated by resolution. In these instances, the only trouble experienced was difficulty in urinating. In the course of time, a permanent stricture is formed, consecutive to the lesion.

More frequently the tumor inflames, suppurates, and opens into the canal, or externally. If it opens into the urethra, and the orifice be in front of the tumor, it may cicatrize without urinary infiltration, but stricture is sure to follow. If the opening of the abscess is situated about the centre of the tumor, or somewhat posteriorly, the cavity is filled with urine, inflammation sets in, and a new abscess forms in the perinæum, followed by urinary fistula.

The common result of contusions is stricture. The slight contusions of the first degree, terminating always by resolution, have the same effect. In these cases the coarctation must be attributed to the adhesive inflammation of the corpus spongiosum. Lymph thrown out into the cells obliterates them, causing adhesion to each other, and also to the membranes enveloping them. When the exuded fluids are taken up by interstitial absorption, the tissue retracts, or appears to be spontaneously atrophied.

If the contusion has affected, to a considerable depth, the tissues, inflammation, suppuration and gangrene terminate the destruction of the contused parts, the loss of substance is replaced by an abnormal, inodular, retractile tissue, which produces organic stricture.

Causes of the second category.—Urethral inflammation.

It is generally admitted that urethral coarctations arise most frequently from anterior inflammations. A. Cooper and Delpech state such to be true ninety-nine times in an hundred. All irritations, internal and external, capable of inflaming the mucous membrane, of maintaining the inflammation, and allowing it to pass into the chronic state, are causes of stricture. Some general considerations on the state of the urethra in blennorrhagia will assist us in appre-

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ciating the manner in which coarctations are produced by it. We must remember in what tissue the inflammation is situated; the phenomena there developed, and how the inflamed parts modify the dimensions of the urethra. Inflammation which produces the greatest number of strictures, is that which extends from the mucous membrane to the submucous tissue; to the internal layer of the corpus spongiosum, and sometimes to this body itself.

The morbid tissue of organic stricture is nearly always superficial, affecting the internal membrane only. But in chordee the cells of the corpus spongiosum appear to be invaded by inflammation. Besides the urethral discharge and pain in micturition, (certain indications of inflammation of the mucous membrane,) there is tumefaction of the canal, and diminution of its calibre—symptoms belonging to inflammation of the spongy layer. However, may not the urethral tumefaction, and the hard chord, painful under pressure, which draws the penis downwards, be attributed to a simple afflux of blood provoked sympathetically by inflammation of neighboring tissues? Is it not hyperæmia or a kind of engorgement? I somewhat believe it, especially when I consider how rare it is to see the spongy tissue participating in the alteration of tissue producing stricture.

In blennorrhagia the calibre of the urethra is undoubtedly diminished. The small jet of urine, pain in its excretion, the difficulty sometimes of introducing very small sounds, and the suffering produced when they are passed, confirm the narrowed condition of the canal. Is this diminished capacity due to spasmodic retraction of the urethral walls, to their prominence from engorgement, or is it from their loss of elasticity? This last hypothesis is the most plausible.

The urethra, as we have remarked, is not habitually a tube. Its properties of extensibility and elasticity permit it to become such during micturition or the introduction of instruments. These properties, however, disappear during inflammation. The state of the urethra in chordee is an incontestable proof of it, and the pain in micturition is an

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argument in favor of our opinion. Authors attribute this pain to the contact of urine—its passage over the inflamed and sometimes ulcerated mucous membrane. We respect this opinion, but think the pain is produced, in great part, by the raising and dilatation of the walls from the wave of urine. If the burning sensation in the mucous membrane was caused by the passage of urine only, the pain should continue till the act is completed, and even increase in severity from the beginning to the end. But such is not the fact. At first the pain is acute, then diminishes in severity and is much exasperated while the last drops are being expelled. I think the pain in the commencement of micturition should be attributed to the sudden separation and raising of the urethral walls—pain at the end of the act, produced by the energetic contraction of the muscles to expel the last drops of urine. It is, then, the loss of extensibility and elasticity of the walls which causes the uniform diminution of calibre during the acute stage of blennorrhagia.

We pass on to strictures which do not follow blennorrhagia immediately. They are more likely to appear when the inflammation has been deep, localized, and remained for a time in the chronic state. Many circumstances may concur to give the inflammation the above characters:

1. One cause of irritation, which, from its frequency, exercises great influence in keeping the inflammation in a chronic state, is the excretion of urine. It not only leaves a baking sensation, but the pressure, friction and dilatation from the column of urine, on the narrowest, least extensible and most sensitive portion of the canal, are sources of irritation which perpetuate the inflammation, impede the absorption of morbid products, and favor their organization.

2. Abortive injections often exaggerate the inflammation, or keep it chronic.

3. It is the opinion of some that syphilitic infection favors the continuation of the inflammation in a chronic state. Hunter, Boyer and others suppose that scrofula, gout, rheu-

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matism, &c. prevent the resolution of the inflammation and absorption of its products.

4. One circumstance, which leads certainly to the formation of stricture after blennorrhagia, is the presence in the canal of ulcerations, or small abscesses of the sub-mucous cellular tissue.

Opinions regarding the frequency of urethral ulcerations have been various. The ancients supposed that ulcerations were always the causes of blennorrhagia. I believe they frequently complicate blennorrhagia, and after a time are followed by stricture.

Such are the conditions which maintain chronic inflammation, oppose its resolution, and favor the development of the abnormal tissue of organic coarctations.

Art. III.—*On the Diagnosis of Typhoid Fever.* By James B. McCaw, M. D. Richmond, Virginia.

The investigations of Louis with regard to the pathology of typhoid fever, have produced a lasting impression on the medical mind, and still continue to modify most essentially the treatment of fever in all its varieties.

The influence of these labors of French pathologists have been more felt in our own country than in England. This arises from the fact that the type of fever prevailing in France, and which was shown by Louis to accompany a peculiar inflammation of the glands and follicles of the intestinal canal, is also very generally met with in this country. But when the descriptions of the pathological alterations of the glands of Peyer and Brunner were received by the English pathologists, they failed to discover those evidences of intestinal disease, so clearly described by Mr. Louis; but on the other hand, they frequently found, accompanying the same febrile condition, serious pathological changes in the lungs and brain. Hence the distinctions between typhus and typhoid fever, which are generally con-

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sidered to signify the two varieties of the same disease. Typhus we may consider the English fever, characterized especially by congestions and infiltrations of the lungs; whilst typhoid fever may be regarded more as the French and American variety, having all the morbid appearance described first by Louis and proven since by the investigations of many other observers.

Rokitansky classes both diseases together, and includes with them the plague or pest-typhus of the southern climates. He considers the *typhous crasis* to consist in a diminution or destruction of the fibrine of the blood, with a preponderance of blood globules. This typhous poison shows its influence, especially on the mucous membranes. In the northern climates it invades the bronchial membrane, producing the congestions and morbid appearances of the English typhus. It prefers, in the central parts of Europe and in America, to involve the intestinal mucous membrane, constituting the typhoid fever of Louis, whilst Rokitansky believes that the same morbific agent, when attacking the peripheral glandular system, produces the pest-typhus which is probably the plague of ancient times.

There can be no doubt that true typhoid fever is a common disease with us. The pathological alterations of the intestinal glands have been demonstrated by a thousand autopsies, whilst the mortality tables of the census of 1850 show that the number of deaths from typhoid fever in one year were more than *thirteen thousand*; yet we are satisfied that there are many febrile affections constantly occurring in practice included under this head, which are not true typhoid fevers, and which are more amenable to treatment than the formidable malady with which they are frequently confounded.

There is no medical man of any practical experience, who does not frequently meet with these low forms of fever, and who has not often felt the difficulty of making an accurate diagnosis between them and the simple idiopathic fever, or the masked remittents of our own state. It would, there-

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fore, be of great use to find out some way to determine this important point, and we will first ascertain what are the forms of fever most liable to be mistaken for typhoid fever, and then the method of diagnosing between them.

Under the influence of the ardent and persuasive reasoning of Broussais, the great body of the profession for many years doubted the existence of idiopathic fever. Broussais declared fever to be always a symptom, and only to be relieved by curing the inflammation which was its cause. The invariable presence of the local inflammation of the intestinal glands in typhoid fever aided in confirming the truth of this statement. It is plain, however, that the views of the profession have changed essentially on this point in the last ten years. The recent lectures of Stokes, lately published in this journal, will show how much that eminent medical philosopher has altered his opinion on this subject since he wrote his well known "Practice;" and there are few now who deny the existence of idiopathic fever, when no local inflammation exists, and which can only be accounted for upon the principles of the humoral pathology.

Typhoid fever itself is now regarded most generally as an idiopathic disease, whilst the intestinal lesions are considered as secondary results. With this disease, however, the inflammation is the *necessary* consequence. Like the small-pox fever, it is obliged to end in an irruption. Hence, typhoid fever has been considered by some in the same light as scarlatina, small-pox, and the other eruptive disorders, and the intestinal complications are regarded as the analogues of the eruptions of the latter diseases.

These simple fevers are often occurring in this country, and when neglected or badly treated, are apt to assume a typhoid aspect. The expression of the countenance, the dry tongue, the duration and obstinacy of the symptoms, lead the practitioner to believe that he has to treat an inflammation of the small intestines, and he does not hesitate to pronounce the disease typhoid fever.

Another variety of febrile disorder, which is very common

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in our climate, is the masked or irregular form of remittent fever. In those parts of the country which are more or less under malarious influences, we often see remittent and intermittent fever so *covered up*, if we may use the expression, that it is difficult to form a correct opinion as to its true nature, and we are satisfied that these doubtful forms of malarious fever are also mistaken for the true typhoid.

Every medical man in active practice has often to treat cases presenting the symptoms we here rapidly enumerate, and which are commonly called the symptoms of typhoid fever. The patient has been not well for some days, and yet is not willing to confess himself sick; but his dry, hard skin, quick irritable pulse, loss of appetite and general feeling of prostration, admonish all around him that there is something serious the matter. His sleep is disturbed with bad dreams, and frequently severe headache occurs at night. There are chilly periods in the early days of the disease, and there are also irregular perspirations, which look like the terminations of the paroxysms of remittent, yet the tongue is dry, fissured and red, and the teeth present the porcelain whiteness characteristic of typhoid fever. Sometimes, instead of a quickened pulse we have the reverse—a slow, measured beat of 60 or 65; and when this slow pulse is accompanied by heavy sweating stages, our experience has been that the fever will be malignant and obstinate. The condition of the bowels varies much. When they are loose and present the true typhoid fever appearance, our diagnosis is less difficult. This stool is watery, yellowish, and having shreddy flocculi floating on its surface, which are the epithelial cells of the intestinal mucous membrane, whilst settling to the bottom of the vessel can be seen a peculiar whitish substance, which is considered by Rokitansky and many other pathologists to be the *typhous exudation*. Some of the most fatal cases of fever we have met with, however, have been free from any tendency to diarrhœa, and in some of them perforation of the intestinal coat and subsequent peritonitis has suddenly occurred, when there had

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existed no symptoms of enteritic disease during the whole progress of the case.

The petechial spots, which are described by all the authors as accompanying typhoid fever, are, when present, to be considered as almost pathognomonic. But we are disposed to think that even in the genuine disease we see the characteristic eruption but rarely; and in our negro population, who are often the victims of this affection, the petechiæ would not be visible. Without pretending to be positively accurate on this point, we think that the eruption of typhoid fever is not found in more than one-tenth of those cases which are regarded by the profession as genuine; and therefore, this prominent symptom is not to be relied on. On the other hand, cases commencing with all the characters of simple intermittent, have run into a low grade of fever, and the splotches and petechiæ of the typhoid disease have manifested themselves.

However the symptoms may vary in the commencement of these diseases, as they progress the difficulty of drawing any distinction between them increases. Lingering on day after day for three or four weeks, and sometimes much longer; the debility more marked; the emaciation extreme; the tongue like wood; the brain wandering, from loss of its healthy stimulus—these obstinate maladies seem at last to exhaust themselves; and in spite of the gravity of the symptoms, it is pleasant to think that recovery is the rule rather than the exception. Indeed, there is no disease we meet with, which we treat, or rather don't treat, with more hope of a favorable termination than this tedious and formidable affection.

Many physicians have evidently doubted the propriety of giving to all these low grades of fever the name of typhoid. Hence we hear of slow continued fever or nervous fever. Yet the practice is always the same as if they had the genuine disease to manage; and finding how apt they are to terminate favorably, we generally resort to the *médecine expectante*, and stand quietly by to witness a spontaneous cure.

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But if we could distinguish between the varieties of disease; if we knew there was frequently no intestinal inflammation, only an idiopathic simple fever, or possibly a marked remittent to contend with, we might then resort with boldness to those remedies which we know can control these febrile conditions; the duration of the attack might be greatly shortened, and the local congestions and inflammations, the consequences of the fever, would be avoided.

We have already said that it was often impossible, from a study of the general symptoms, to arrive at a satisfactory opinion with regard to these fevers. The previous history is often useless, for the malaise, which generally precedes the period of our visit, tends to throw additional confusion around the case.

If the patient has been exposed to a malarious climate, and his first symptoms were paroxysmal; if the bowels are constipated, and there is pain and torpor of the liver, with a jaundiced skin, we may suspect remittent fever; yet, we should remember that typhoid fever proper may be modified in a miasmatic atmosphere just as pneumonia, dysentery and the other diseases; and it is possible that the inflammation of the plaques of Peyer may exist, and yet mixed with the true disease there may be a certain amount of the malarious poison.

Some of these cases are more active and acute in their career. The pulse is full and the headache intense; the skin hot, with great thirst; the bowels being regular or a little constipated. Such symptoms often lead us to adopt more energetic measures, and are frequently cured in a few days.

We may fear, however, that we have the genuine disease, when diarrhœa shows itself early; if there is abdominal tenderness and tympanitis, and if upon pressure there is the gurgling of fluid to be distinguished in the right hypogastric region. Any irritability of bladder is a suspicious symptom, showing that there is some grave disease in the bowels which is causing a sympathetic irritation of the vesical organ. We have seen this symptom so often, in cases end-

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ing fatally with perforation of the intestine, that we mention it here especially. It is sometimes so great as to cause constant and most painful priapism.

There is, however, one certain evidence of the existence of true typhoid disease with inflammation of the Peyerian glands and follicles of the intestines; and this evidence is to be derived from an examination of the stools of the patient.

We have already described the typhus stool as a watery, yellowish fluid, with the epithelial mucous cells floating on its top, and a whitish molecular substance, falling to the bottom of the vessel containing it. Those who have seen much of this fever know the stool from its general appearances. It is quite free from fœcal smell, and has a faint, mawkish sweet odor, also noticed in dysentery.

The peculiar white substance is pathognomonic of typhoid fever, and when the stool of the patient is examined with the microscope, can be readily distinguished. In the bronchial typhus of the Irish and English hospitals, this exudation is present in the expectoration. The importance of the microscope in the diagnosis of disease is becoming more apparent every day, and we have an evidence of its value in determining a difficult point in the diagnosis of these very common affections.

The accompanying plate will give an accurate idea of the appearance of the intestinal follicles when infiltrated with the typhus substance, whilst the microscopic diagram of the typhus stool exhibits the peculiar exudation, which has been thrown off by the ulcerative process.

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Figure I is a portion of the intestine of a young girl who died of typhoid fever on the 15th day. It exhibits the plaque of Peyer surrounded by unbroken mucous membrane. The glandulæ composing the patches are deprived of their covering of mucous membrane, and the typhoid exudation has been partially emptied. The surrounding mucous membrane still retains its villi. Natural size; taken from Gluges Atlas of Patholog. Hist.

Figure II. This is the characteristic stool of typhoid fever, when examined by the microscope. The evacuation is yellowish and watery, mixed with epithelial cells and many white granules, which are the typhoid exudations from the glands of Peyer. The typhoid substance is seen aggregated together in large compact clusters—some of the clusters consisting of a fine amorphous, molecular deposit. The figure also shows the oblong epithelial cells, and a few blood and pus corpuscles, besides several large crystals of the triple phosphates. The biscuit-shaped formations on the left, at the lower margin of the drawing, are dumb bells of the carbonate of lime, which are often observed in typhoid stools. This beautiful plate is taken from Dr. Otto Funke's Atlas of Physiological Chemistry; Cavendish Society's edition.

Figure III. Here can be seen the spherical corpuscles of the whitish typhoid substance, which has been thrown off from the glands of Peyer—magnified 255 diameters. From Gluges' Atlas.

The fact granted, that many of the so called typhoid fevers are not the true malady which is portrayed in our plate, and the difficulties of distinguishing between them having been acknowledged, whilst the characteristic stool just described, although it settles the doubt, still occurs so late in the disease, that we can derive but little advantage from the information we thus obtain, we would suggest, in conclusion, a method of treatment which may aid our diagnosis in the early period of the fever, and frequently enable us to cut short an attack, which, if left to itself, would not terminate for many days.

To prescribe a remedy before we know what is the matter, may be unphilosophical, but we are compelled to resort to this method very often. It may be called the method by exclusion; and we may prove that a disease is *not* what it might be mistaken for, and thus obtain a correct idea of what it is. A considerable familiarity with the value of this suggestion enables us to recommend it with the more confidence.

Whenever we see a patient in the condition we have described, with the peculiar low type of fever, dry tongue

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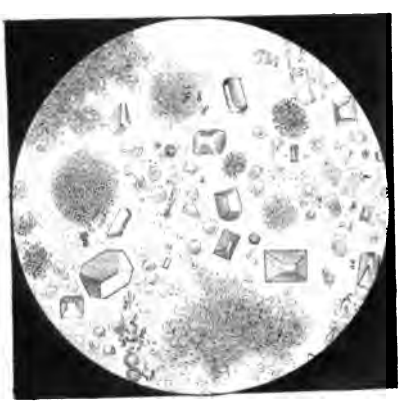
Fig 1.

Fig. 3

Fig 2



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and skin, loss of appetite, and other marks of this prolonged form of disease, we at once use the sulphate of quinine in full doses, and rapidly pushed, until cinchonism is established. The sedative powers of this medicine will be manifest, and if the disease is *not* complicated with intestinal or other local inflammation, a rapid relief of the febrile symptoms will occur. The pulse will come down to a normal standard, the skin becoming soft and moist, the fever will be cured; and this result will follow the practice just indicated, very frequently. We think at least one-half of the "nervous fevers" of this city and throughout Eastern Virginia, can be arrested promptly and safely by quinine. That the proportion would be as large in our fevers of Western Virginia, where the true typhous poison is very commonly seen, is doubtful; but the experiment is justifiable. It is the "*experimentum crucis;*" and settles the diagnosis. Let the genuine disease be in existence, and the quinine may reduce the fever and even induce perspiration and an amelioration of the symptoms, but it cannot permanently control the disease. The heat of skin will return, and the tongue remain hard and dry; the listless expression and feeling of prostration will increase. Beware—your remedy is powerless. Nay, it will, if pushed too far, cause a local irritation of the intestinal canal, and add to the trouble which it cannot allay. We can, under such circumstances, pursue the usual practice; and by good nursing, a bland and nutritious diet and repeated large and superficial blisters to the abdomen, the fever can be conducted to a happy termination.

If the quinine is administered at all, it should be used in the early stages, and in full doses, until its specific effect is produced. If we wait until secondary inflammations have followed upon the repeated congestions of fever, we will assuredly fail in this abortive practice. Ten grains, twice or thrice daily for three days, will test the utility of the drug in each case. After the fourth dose generally the symptoms of cinchonism supervene. The pulse gets full,

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slow and soft, and the skin is bathed in perspiration. If the fever is uncomplicated, the change for the better will be wonderful. If we have typhoid fever to deal with, the remedy will fail, and we should withdraw its administration at once.

ART. IV.—*A Case of Ileus, or Intussusception of the Bowels, Manifesting no Symptoms during Life.* Reported by ALFRED B. TUCKER, M. D. Professor of Materia Medica and Chemistry in Winchester (Va.) Medical College.

Wesley, a servant child, belonging to Mr. T. æt. 2½ years, well grown, generally delicate, has for some time been suffering with disordered bowels, accompanied by fever, for which appropriate treatment was employed.

Sept. 29.—Called to see Wesley, who is laboring under symptoms of dysentery; has had many discharges during the night of pure blood; pulse regular and not accelerated; skin moist and cool; absence of all febrile symptoms. Ordered ℞ Hydrarg. sub. mur. grs. viii; pulv. opii. grs. i. Div. in pulv. No. viii—Sig. one to be taken every two hours. Evening—No change; discharges from bowels still continue; continued pills, and ordered injection of starch and laudanum.

Sept. 30.—Patient somewhat better; pulse and skin the same, discharges less frequent and bloody; complains of but little pain. Ordered one grain of calomel every three hours, instead of powders.

Oct. 1.—Patient still the same. His condition changed but little, and the treatment varied only slightly, until

Oct. 6.—Some fever; bowels much distended with wind; no pain. Ordered ol. terebinthinæ, 5 gtts. every two hours.

Oct. 7.—Patient weaker; tympanitis increased. Continued turpentine, and gave one grain of quinine and calomel each, every four hours. Wine and milk ad lib.

Oct. 8.—Treatment continued. Tympanitis increased,

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and patient weaker; gave injection of nitrate of silver, (1 gr. to ℥ i of water.) Evening—No change. Continued treatment, and repeated injection.

Oct. 9.—No better; no action on the bowels. Treatment continued.

Oct. 10.—Patient died to-day.

Autopsy fifteen hours after death.—Abdomen examined. Stomach healthy, but much contracted; liver healthy; no morbid lesions of upper part of small intestines; the lower part, however, with the whole of the colon from the caput coli to the sigmoid flexure, much inflamed; at the junction of the transverse and descending colon, was a tumefied portion of the gut, into which the finger would pass, as into a glove; this was suspected to be intussusception, and was removed for closer inspection. The colon above this point was much distended with flatus, while the rectum was empty. Upon examination of the removed portion, it was found that about three inches of the gut had passed down into the portion below it; no adhesions had taken place between the sides; the invaginated portion was much inflamed and thickened, and when first seen had more the appearance of a clot of blood than anything else; the outer bowel was comparatively uninflamed.

Remarks.—This case shows that a fatal disease may exist in the body, without showing its presence by any decided symptoms. There was no discharge obtained from the upper bowels during the course of the disease, but this is so constant an accompaniment of dysentery, that it gave rise to no suspicion.

Did it not give rise to the inflammation of the bowels? Beyond a doubt, we believe, for there were no evidences of inflammation below the invagination, all being above it. The transverse colon immediately above was abnormally enlarged.

How long had this condition existed in the bowels before death? If our view above is correct, certainly since the dysenteric symptoms made their appearance. In thirty-five

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cases collected by Dr. Thompson, (Edinburg Med. and Surg. Journal,) the average duration of the disease was between four and five weeks, so that it is possible that the disease existed for some time.

Is it not possible that many fatal cases of dysentery and enteritis are caused by the existence of this disease? As there were no symptoms in this case, so it may be latent in many others, and be really the cause of death.

If it had been discovered during life, assuredly no means would have been available in checking the disease. No remedy in the materia medica has yet been discovered sufficient to overcome the obstruction, and it must as yet be viewed as one of the opprobria of medicine.

LECTURES AND SELECTIONS.

I. *Introductory Lecture to the Medical Session of the University of Dublin.* By William Stokes, M. D. Regius Professor of Physic.

I have been directed by the provost and senior fellows, to open the medical session by a public address. This proceeding, which is a novel one, is not without its meaning. It implies the wish of the heads of the University to make known, through their proper officer, the steps which they have taken to promote the schools of medicine and of surgery, within their walls, and the views which they entertain, as to the advancement, not only of these institutions, but of medical education in its broadest sense.

Our profession, gentlemen, has long been in an anomalous position. While its more successful members may attain wealth, and rank, and consideration, the mass is not on an equal footing with that of the church or the bar. I will here only indicate one of the many causes of this state of things; it is that so many of us have received but the special or exclusive education. It is not so in the sister professions. And the evil has, in part, arisen from the fact, that in former

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times the study of medicine was not sufficiently fostered by the great universities. The teaching of medicine passed in a great measure into private hands, and licensing bodies sprang up, who were content with the merely special education. The result was natural. The student, while yet a boy, commonly uneducated even for his age, knowing nothing of the world or of the requirements of society, is sent to work in the dissecting room, or to sit on the benches of a medical theatre, to listen to lecture after lecture on different subjects, until the time come when he is enabled to pass an examination. At the end of the fourth year, the license is obtained, and he enters the world, too often unfit to take his place in society, or support the rank of his profession. He may be, and generally is, the son of a gentleman, but he has not received the education of a gentleman. Such a man is often soured by finding himself deficient in the knowledge which society requires. He rails at the treatment of his profession by the public, while he should feel that much of what he complains of, is but the result of the vicious system to which he was a victim. Yet such a man is better off than he who has sprung from the lower ranks, and who has never been afforded the sole means by which he could take and keep his place. For he often wants those refinements of feeling and thought which are often hereditary, and which to a certain degree would compensate for a proper mental culture. To him the want of a liberal education is a bar which it is hardly possible to get over.

I can speak on this subject with confidence, for I have now been more than twenty-five years occupied in the teaching of clinical medicine, and I know that some of our students have obtained their diplomas from various licensing bodies, without possessing the knowledge indispensable to a common clerk. I shall not soon forget an observation made to me by a gentleman, then high in office, and head of one of the departments of the public service. "We got," he said, "from Ireland, some of our very best and very worst men." I asked him in what the second class were wanting; he answered, "in conduct and in common letters."

Gentlemen, you have adopted medicine as your profession. Let me ask you, have you thought on the nature of the road you are about to travel—do you expect to move onwards as on a beaten path, like machines and not like men? Do you look at your profession as a mere means of livelihood, to be reached by a course that can lead only to mediocrity, or will you strive to fit yourselves for the highest place, by a full moral and mental culture? I trust you will. We

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hear of our profession being overstretched: but remember that saying of Swift's, that "in every crowd there is room, over their heads." By these means alone will you obtain self-respect. Avoid the lower, qualify for the higher aim. Reject as false, and utterly mischievous, whatever you may be told as to the inutility of a broad and liberal extra professional education; you are not going to a trade, but to become members of an honorable profession, and, therefore, it behooves you to consider whether you will sacrifice your chances of rank and success, by following those advisers who tell you, for their own purposes, that an exclusive education will serve your purpose. I warn you against such a doctrine. A few years will show you that it is contrary to the spirit of the age. Do not believe that with your minds unopened, unrefined by literature, untrained to accurate reasoning, with your tastes undeveloped, and your habits untutored; the mere fulfillment of your medical or surgical curriculum, will qualify you to support the honor of your profession, or place you in the class of the workers and pioneers of a progressive science. No, you will not go to a profession as an ignorant man to a handicraft; you are to be associated with men of education, therefore you must not be inferior to them in education; you are to support the dignity of a noble profession, therefore your mental powers, and your moral perceptions, must be cultivated and exalted; you are to be placed in a position of all others the most fruitful in opportunities of doing wrong to your brother in the dark, and of obtaining wealth by practicing on the credulity of mankind; therefore you must cherish the most delicate sense of honor, and so train yourselves, that your conduct shall be based, not so much on the fear of the consequences of wrong, as upon the perfect love of that which is right.

But let me not be understood as conveying any slight on the members of our profession who have been condemned to the mere education of the curriculum. This, however, I will say, that their character has been made by one branch of study. It was not made by the practice of repeated anatomical dissections, nor by listening to triple courses of lectures. It was made in the hospital. In its wards, the student learns that which cannot be taught in the dissectiug room, or in the theatre. Here—where disease in every shape, and the suffering of his fellow-man in every degree, surround him, and mutely, but imperiously, ask for help—he begins to do for himself that which he had been hindered doing before. He learns to reason, to teach himself to act

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and to discover. But he does much more. The kindlier feelings of his heart are stirred, and he is so trained to works of charity and mercy, that their practice becomes at last a second nature. He may be rude, unlettered, but he not only bears with him into after life that which is the characteristic of our profession, the wide spread and lasting exercise of that virtue which the inspired apostle denotes as the great mark of the christian character; but, he acquires that moral courage by which, at the call of duty—or of mercy, which is his duty—he learns to despise danger and to meet death, whether it come by the pestilence or the sword.

Gentlemen, medicine is essentially a progressive science, and avails itself of almost every branch of knowledge in its progress. It has been objected to medicine, that it is not an exact science, and a slur is attempted to be cast on it by those in whose minds the ideas of the inexact and the erroneous, or at least the uncertain, are confounded.

Medicine is an inexact science; but this is no reproach. By this very character it enters into fellowship with the most noble of human enquiries—with those which have for their object the relations of the created to the Creator—the future state of man, his moral and his intellectual nature.

Remember what it is that constitutes the bases of the exact sciences—number, force, weight, form, arrangement, chemical constitution, and so on.

The mathematician, ascending from the simplest idea, can develop new powers almost indefinitely. The astronomer measures and weighs the heavenly bodies; and the physical philosopher, dealing with light, and heat, and electrical action, can bring either pure mathematics or geometry so to bear in his researches, as to enable him not only to explain, but to predict. Thus our own Hamilton predicted conical refraction; our own M'Cullagh was in the path to discover it by another method; and Leverrier and Adams declared the existence of a planet which eye had never seen. The chemist has analysis and synthesis at his command, and even the anatomist and geologist have their constants to start from. They have little more to do than to observe, to reckon, and to compare. They deal with what has been, and their principles, once determined, are little likely to be disturbed.

But we have to do with something which cannot be measured or weighed; something, too, in which experiment can only be used within narrow bounds—an element whose nature is yet unknown, fleeting in its action, and every day producing new combinations, not merely new because they

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were never observed before, but really new as appearing for the first time.

All this will help us to understand why we have not yet a theory of medicine. But let not our zeal be therefore diminished: we are young in the world. Let us firmly believe that, at the appointed time, when the required amount of facts have been faithfully recorded, they will, by one of the great properties of truth, crystallize spontaneously into a system and a law.

In the mean time, let us never forget, that every connection that can be established between the mathematical and physical sciences and medicine, will impart to it more or less of certainty. It was long ago proposed by Graves, that, with the view of discovering the laws of epidemics, medical observatories should be established in various parts of the world, in which reliable observations should be made, not only on the history and character of disease, but on all the co-existing conditions of temperature, of chemical and magnetic action, and the hygrometric and meteorologic states. This great idea has since been put forward by Humboldt, who, in speaking of epidemic diseases, observes that we shall probably remain in ignorance of their nature and causes, until all the laws of terrestrial magnetism and meteorology are determined. What better argument could I use in favor of a scientific education?

Gentlemen, I have in this lecture alluded to but a few of the causes which have tended to retard the progress of medicine; but there is, in the very nature of medical science, a power and a virtue which will cause it to triumph over all those influences which an unenlightened system has brought to bear upon it. A time will come when it will take its proper place, and what that place is to be, we can anticipate without difficulty and without presumption. The physical and moral natures of man are mysteriously and inseparably united: and if to save the erring soul, and to elevate the moral condition of man, was thought worthy of a special revelation, can we doubt that the science which has for its object the well-being of man in his physical state, so intimately mixed up with his mental and spiritual life, will not yet occupy a foremost place? Let us not, then, be disheartened; remember that the human family is still young upon the earth, so young that not a monumental inscription of one of us has been yet written on any of the mighty tombstones with which nature has overspread the earth. Let us have an unshaken belief in the ultimate harmonizing of the truths of religion and of science, a harmony which,

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at this moment, doubtless exists, if we could but see it; and let us further believe, that when those influences which enslave the minds and the bodies of men have faded away before God's light, we shall see our science taking a foremost rank, only inferior to that which deals with man's future existence, and man's future hopes.—*Med. Times & Gaz.*

II. *Notes on the Application of Statistics to Questions in Medical Science, particularly as to the external Causes of Diseases.* By W. P. Alison, M. D. Edin. D. C. L. Oxon. Emeritus Professor of Practice of Medicine, Edinburgh.*

It has so frequently and so plausibly been urged against all the enquiries and studies which are termed statistics, that the form of such reasoning may be applied to the support of almost any proposition, that it becomes an object of very considerable importance—in the view of any one who is truly convinced of the importance and frequent practical application of such enquiries—to point out the circumstances of any question, or departments of any science, in which this kind of information is truly requisite, and the conditions under which it may be trusted. This is especially true of the science of medicine, because there is one great department of that science—that which we term etiology, or the doctrine of the external causes of diseases, in which our knowledge is acquired almost entirely in this way. It is in very few cases only, that our knowledge of the essential or intimate nature, either of diseases or of the powers in nature which excite them, enables us to form any anticipation of the effects of those powers; and it is simply by *empirical* observation—facts observed and recorded, and the frequency of their recurrence noted, although not explained, *i. e.* it is by the mere *force of numbers*, or by *statistics*, whether stated exactly in that form or not—that our information on that subject, and practical rules for the prevention or treatment of diseases, founded on that information, are acquired.

The importance of this observation will not be denied, when we remember that this is the department of medical science which is truly of the greatest practical importance. The knowledge of the external causes of diseases, is that

* Read to the Statistical Section of the British Association at Glasgow, Sept. 1855, (by Sir Archibald Alison.)

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which leads most directly to their prevention; and to the preservation of those lives especially, from which the greatest amount of labor of all kinds may be obtained, and which are, therefore, generally regarded as most valuable to a state.

In the course of the present century, improvements have been made in medicine, which will bear comparison in their practically beneficial tendency, with those which have made this age and this quarter of the globe so illustrious, as regards the applications of any other sciences to practical purposes. These have been almost exclusively in this department of medicine, and may be truly said to rest, as yet, almost exclusively on statistics; anticipating, probably by several ages, any information within the power of the human race, as to the intimate nature of the phenomena which are thus recorded. It is simply by the force of numbers, attesting the simple fact of a disease, easily recognized, showing itself in many persons within narrow limits of time and space, and there only, that we have learnt how the poison of small-pox may be diffused; and that it may be disarmed of its power over the human body, by being first taken into, and modified by its passage through, the body of the cow, and then applied, in almost infinitesimally small quantity, to the human body itself, and there exciting a certain specific inflammatory process, absolutely devoid of danger, and incapable of communication through the medium of the air, and, what is still more inconsistent with the knowledge we have of other changes in nature, that this modifying and preserving effect on the human body, produced in infancy, continues equally perfect, in a great majority of cases, after 60, 70, or 80 years, if life shall continue so long, *i. e.* after we are certain that the body on which it has been produced, has been repeatedly *worn down and built up again*, so that the poison introduced into the structure in advanced life, can no more be said to come in contact with living matter which has gone through the process of vaccination in infancy, than, according to the paradox of an ancient logician, a man can be said to have bathed twice in the same water, because he has bathed twice in the same river.

So also it has been simply by empirical observation, and, therefore, by reference to statistics, that we have acquired within these few years, information touching the extension of another epidemic, less frequent, indeed, but attended with peculiar interest, and often with extreme fatality, the puerperal fever, which enables us, with almost absolute certainty, to predict that its propagation, after the manner of

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an epidemic, may hereafter *always be prevented.* For I believe I may safely assert that the statistical observations of Dr. Semmelweiss, in the great lying-in hospital at Vienna, where upwards of 6,000 births take place in a year—being in exact accordance with what has been seen in this country, have unequivocally shown:

1. That this disease is, in fact, a case of the diffuse inflammation or erysipelas, attended with the same peculiarities in the extension of the inflammation, and in the nature of the effusion, and the same variation as to the nature of the accompanying fever in different epidemics.

2. That one immediate exciting cause of this form of inflammation which may always be suspected when it prevails epidemically is the *cadaveric* poison—often evolved, during the decomposition of the human body, from whatever cause death may have taken place; but during one stage of that decomposition only, viz: that early stage during which the *post mortem* inspections for the investigation of the cause of death are most frequently made.

3. That when this form of inflammation assumes the form of epidemic puerperal fever, the mode of its transmission from one patient to another, is by accoucheurs or nurses, themselves in good health, but to whose persons or clothes minute portions either of the effluvia from others already affected, or of this cadaveric poison, have become attached; and

4. That when the obvious precautions are taken to prevent any persons to whom, in either of these ways, the poison can have thus adhered, from acting as accoucheurs or nurses until effectually purified, no "epidemic extension" of puerperal fever is seen.

It is seldom, no doubt, that this last proposition can be submitted to a searching scrutiny, as in the year 1846, at the Vienna hospital, under Dr. Semmelweiss, when the number of deaths, after these precautions were adopted, was diminished by no less than 400 in that hospital alone, and in a single year; but when it is added, that each of the propositions above stated is quite in accordance with observations made and recorded (although the bond of connection among them had unfortunately not been duly observed) in this country, whenever puerperal fever has been epidemic, whether on a large or small scale—so that it would be easy to collect statistical evidence of the same kind from every such epidemic, wherever any record of the facts has been left, to establish each of these propositions—the force of this statistical evidence becomes such as to justify the sanguine anticipation above announced.

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Our information as to the mode of extension of the malignant cholera, is, unfortunately, as yet less certain or precise; yet I think we may say it is so far advanced, that we may entertain a confident hope of its being very soon such as to disarm any epidemic cholera, in this climate, at least, of all its terrors.

I have great hopes that we shall soon have statistical evidence to establish a proposition which no other evidence known to us could justify our admitting, but which, if established, would not only reconcile most of the conflicting statements on this subject, but serve as a guide to almost complete security from any epidemics of this disease in future, viz: that the poison of cholera, like other known animal poisons, is developed during the decomposition of the animal matter, the appearance of which is most characteristic of the disease—that which constitutes the "rice water stools"—but only in a particular stage of that decomposition, not immediately after its formation in, or discharge from, the body; and again, not after the decomposition has gone to a certain length. All this can be proved only by statistics, but is quite susceptible of proof in that way. The last proposition above stated—that no poisonous property is attached to any part of a body dead of cholera, after a certain stage of decomposition has been passed, seems nearly ascertained from the number of instances in which dissecting rooms have been supplied with bodies of persons dead of cholera, for many weeks together, without any of the students attending them being affected. That the peculiar matter of the cholera evacuations, during its decomposition, perhaps especially in dry air, has a peculiarly poisonous quality, was, I believe, first suspected by Liebig, and partly by the analogy of the poison already mentioned, causing erysipelatous inflammation and puerperal fever—and also of the sausage poison, repeatedly observed on a large scale in Germany, which is developed during a certain stage of the decomposition of the animal matter of those sausages, and disappears when their putrefaction has so far advanced. That the cholera can be communicated to animals by inoculation with this matter, of the peculiar rice water stools, has been sufficiently proved by the experiments of Dr. Lindsay in this country; and if a few more experiments shall give results similar to those of M. Thiersch, at Munich, I think we may assert that the proposition above stated is statistically proved. "Dr. Thiersch collected the intestinal contents, or the evacuations, of cholera patients, and let them decompose under the influence of air and heat. From day

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to day he dipped into this matter pieces of filtering paper, which he dried, for subsequent experiments on white mice. Two of these animals at a time were exposed to infection for four days, by having a square inch of the filtering paper, thus prepared, moistened with water and mixed with their food. Each mouse took thus 1-2000th gr. daily. The results were as follows: The preparations from the matter during the first day of decomposition were inocuous. To this succeeded a period of from six to nine days, during which decomposition went on, and preparations from the matter in this second period of decomposition caused disease in 30 out of 34 animals, and death in 12 of the 34. The symptoms were peculiar and characteristic. The hair fell off, the ears dropped, there was languor, then discharge from the bowels, first of white, then watery matter, the urine lost its smell, then was suppressed, the appetite became depraved, so that the animals would fill their stomachs with wool; there was no apparent sickness, but such tonic muscular contractions that they seemed dead some time before death. On dissection, accumulation of blood in the vessels of the small intestines was invariably found, their contents watery, with abundant epithelial flakes (just similar to those found in persons dead of cholera;) the cortical substance of the kidneys passing into fatty degeneration, the bladder empty, the blood and the contents of the intestines answering to the test of ready mixture with amygdalin, as in the cholera of man—which will not appear in the healthy animal. To this a third period of decomposition of the matter under trial succeeded, in which these poisonous effects were very slight, or not observed at all." See Medical Times and Gazette, Nov. 25, 1854.

We have already a statement by Dr. Budd, of statistical observations made on villages in England, where the entrance of cholera appeared to be prevented by such expedients as these observations immediately suggest, for receiving the rice water evacuations of cholera patients on linen or cotton, and burying or burning, or otherwise effectually destroying their substance, during that period, thus indicated, after they have been passed, and before they have entered on the morbific decomposition; and we have sufficient statistical evidence of the importance of another measure—which was first adopted, I believe, at Edinburgh in 1832, and has since been recommended by the board of health in London, and adopted in different places, although not so generally or satisfactorily as could be wished—founded on the merely empirical statements I have made as to the

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communication of the disease from the sick to the healthy, and its apparent adhesion to particular, often very limited, localities, viz: the establishment of houses of refuge for the reception of all inhabitants of houses or rooms which might become infected with cholera in any town; not themselves affected, nor required for the care of the first cases that might occur. Here such persons might be lodged in pure air, regularly fed, preserved from cold and from other (frequently concurrrent) exciting causes of the disease, and treated with due attention to cleanliness, immediately on any symptoms of cholera showing themselves. The London board of health report that they had information of 1691 persons taken into these houses of refuge, from rooms where there were patients in cholera, and of these only 33 became affected with cholera, and only ten died. In cases of which I was myself informed, in Edinburgh, at Glasgow, and at Oxford, during different epidemics, 1010 persons were admitted from sick rooms into such houses of refuge, of whom 40 took the disease, and 15 died; whereas the experience of Dr. Hamilton of Falkirk, of 251 cases of cholera appearing in 86 houses, where no such means of separation existed, gives only a fair idea of the extent to which successions of cases will often be observed, in confined air and dirty districts, possessing no such resource.

I am happy to say, that so far back as November last, having written to Dr. A. Smith, at the head of the medical department of the army, on the subject of the decomposition of the rice water stools, as the probable cause of the propagation of cholera, and of districts becoming tainted with the poison of that disease—I was informed by him that he had directed the attention of the medical officers in the Crimea to the facts now stated, so that if the disease shall appear in a malignant form in that army, we may hope for at least accurate and truly statistical information as to the reality of that opinion.

Again, as to the yellow fever, so frequently becoming epidemic in the hot climates, although we cannot boast of having acquired information either as to the nature of its cause, the essential character of the morbid change it produces, or the power of any remedy over it—yet by simply empirical observations, i. e. by statistics, we have information to the following effect, as stated in reports in Germany and France, on enquiries conducted by order of those governments, on a large scale, and considered by committees containing the names of Humboldt and Dupuytren, that it is a disease endemic, almost exclusively, "in districts nearly

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on the level of the sea, never appearing beyond 48° of north latitude, nor without a previous temperature of 72°—only in certain circumstances propagated by contagion," but when epidemic, always confined strictly to certain localities; so that the practical rule of immediately evacuating, i. e. removing all the inhabitants of places where it is declared to exist, and has formerly prevailed, is incontestable, and "of such proved utility, as will always justify its rigorous execution."

Without dwelling farther on the effects of malaria, in this or any other climate, which we can only expect to be satisfactorily explained when pathology shall be considerably more advanced than at present, I may merely add, that such collections of facts have been made, and are now frequently repeated, as to the places and circumstances in which it arises from the earth, and the laws according to which it extends and multiplies—considered merely empirically or statistically—as we may confidently expect to be successful in disarming this cause of disease likewise of its terrors, long before the nature of the change produced by its action on the living body, or the rationale of any line of treatment of those who may be affected by it shall become known.

The cure of epidemic scurvy, resulting in different cases, as is now satisfactorily established, from different deficiencies in the diet habitually taken—the efficacy, therefore, of different kinds of diet in counteracting this form of disease—still more remarkably, the power of small quantities of vegetable acids in producing the same effect, and the extraordinary rapidity with which such changes of diet, and these acids, will produce their effect—may also be stated as examples, on a large scale, and of the most satisfactory kind, of what is generally called the power of art over a most loathsome and virulent disease—but in reality must be regarded, now and probably long after our time, as results obtained by simply empirical observations of the course of nature, statistically arranged, fortunately facilitated by so many of the subjects being organized bodies of men; and which have distinguished the present age to a degree, which those who are not familiar with the medical writings of the last century will hardly conceive.

We have good reason to hope, that enquiries now on foot as to the external causes of scrofulous, or of what is now usually called tubercular disease, including pulmonary consumption—an enquiry which we must perceive to be more complex, and in which the operation of various causes must be recognized—will be effectual in pointing out the means

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of counteracting that tendency in a very large proportion of cases of persons liable to it—simply on the principle of empirical observation, enlarged and arranged in the form of statistics, long before we shall have information as to the essential nature of the vital process, or mode of operation of the causes in question.

In illustration of this, I need only mention two facts, recently ascertained on so large a scale that we have no doubt of their truth and importance, and which, even at present, may be said to be guides to successful practice in many cases only recently thought hopeless, although all that was previously known on the subject was certainly rather adverse than favorable to the supposition that they would ever be established. These are—1. The good effect of the cod liver oil—if not of other animal oils, on many cases of tubercular disease, in their early stage—provided only that it can be retained on the stomach to the extent of an ounce and a half or two ounces daily; and 2. The almost complete exemption of the inhabitants of the Faro islands from tubercular disease, notwithstanding that their climate, as regards cold and damp, is exactly that which in this country has been thought most favorable to it.—*Edinburgh Med. & Surg. Journ.*

III. *Duties of the Practitioner to the Dying.* By the Editor of the Medical Times and Gazette.

In our last number we endeavored to show the unpleasant impression made by some members of our profession when they presume to tell patients that their days are numbered, and when patients dare to live on and get well in spite of the prophecy. There can be but few of our readers who are not acquainted with some case or other which has been too hastily condemned in consequence of cavity in lung, or imperfect valves of heart, or albumen in urine. We speak not merely of gloomy anticipations, which have not been realized—nor yet merely of suspicions of danger, plainly, yet cautiously intimated to the patient or to his family; but we speak of positive, peremptory statements that Mr. A or B cannot live six weeks, or whatever the term Dr. Rhadamanthus may be pleased to allow. Of course, patients so sentenced give up their faith in the profession, and lend a willing ear to any quack who may promise them relief

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Nor is this to be wondered at. It is a serious injury to the profession, but for one which the profession itself is to blame.

But the whole question involved is a very serious one; and as we write for men whose avocations are daily exposing them to the possibility of error, we make no apology for taking up a few moments of their time in discussing the right principles of action under certain circumstances.

In the first place, we are bound to set before us, in its fullest extent, the idea of LIFE as the most precious of all gifts of the Creator. It is the gift which includes all others. It is, so far as we know, the only opportunity which man has of improving his own moral being, or of contributing to the carrying out the designs of the Creator. No life whatever can be set at nought, nor can we venture to call it of no possible service. Life, therefore, is to be upheld, maintained, struggled for, under all circumstances, by the practitioner of medicine; and it is his very first duty to avoid all things which may shorten it, even for an hour.

Equally is it the duty of the patient himself to cling to life with the most unflinching purpose. It is his time of work and improvement; and, till all possible work is done, and all further moral improvement impossible; till the body has reached the term of natural decay; till all the duties of life are accomplished, we hold that to desire death, or not to desire life ardently, and not take every rational means of prolonging it, is an offense against religion and nature. The fear of death and love of life were implanted in our bosoms, like all the other instincts, for the wisest purposes.

Here we must bestow a passing word of condemnation upon that prevalent idea of the peculiar sanctity of early death—the detestable and unnatural notion that consumption, for instance, is *la mort des élus*, the fate of those whom the Almighty regards with his most especial favor. All the analogies of heaven and earth cry out against a doctrine so shocking. Work—work, inward and outward; work in subduing his own passions; work in subjugating the earth to the use of man; work in the rearing of a succeeding generation, and in promoting the improvement of the human race: this is the real lot of man, and he is most blessed whose lot is in this respect the richest. But in what we say, do not let us be understood as denying the "sweet uses of adversity." Some there are to whom Providence has denied active work, and yet whose example of suffering patiently the loss of health and of all the enjoyments of youth, is felt as a most blessed influence upon all around them. Meekness in suffering is exquisite; but yet the great ends of

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our being are and must be carried on by the work of the strong and healthy. To resign life without a murmur is good; to employ it actively is better. View it in what aspect you will, it is life which is our most precious possession; resign it we must, without a murmur, when called on; but to despise or throw it away, or not to desire to keep it, is a sacrilege. *Vivens! vivens! ipse confitebitur Tibi* is the soul of the *religio medici.*

Holding life, then, in this most sacred light, we are prepared to lay down the exact motives which should influence the practitioner, when it is in danger, or supposed to be so.

For example: we believe that it is a positive duty, never to be neglected, to remind the patient, or those about him, (if a reminder is necessary,) of the expediency of availing himself to the utmost of the consolations and offices of religion. But this should not be done merely because the patient is likely to die, but simply because, being ill, he cannot seek the offices of religion in public; because he has time to attend to them, and requires the comfort they afford; and more especially because they afford the means of living on—if life be spared—more usefully and happily. We repeat, that religious offices should never be degraded into mere nostrums for the dying; the clergyman coming when the physician gives up the case; as the ancients put money into the mouths of the dead to propitiate the ferryman of Hades: but they should be resorted to as means of better life—as the daily bread of the living, and not merely as the viaticum of the departing.

So let us imagine the case of a person ill, or hopelessly ill. It is the duty of the physician still to adopt every means for his benefit. But is he to inform the patient in every case that there is little or no hope?

We can conceive that such a course is justifiable if the patient willfully and recklessly neglect to settle his worldly affairs, or persists in a course of profligacy and wickedness. Scarcely otherwise

"Gravi me terrore pulsas, vitæ dies ultima."

So said a poet, with whom our readers ought to be acquainted, and so says human nature. To tell a man that he must be cut off, inflicts a pang sharper than that of the knife, and it should not be done unless some great end is to be attained. We are not meaning to imply that the physician is to conceal *danger* from the patient's friends, or to state anything positively untrue; but we do say, that his

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paramount duty of preserving life to the utmost may render it imperative on him not always to tell the whole truth to the patient, if, by so doing, he lessens by one jot or tittle the chances of his recovery. For our own part, we should always desire to know if our own life were in jeopardy, and should feel bound to give the same information to any patient who desired it, and was able to view the matter in its proper light. But there are many to whom such an announcement would be dangerous and unjustifiable.

CHRONICLE OF MEDICAL SCIENCE.

PHYSIOLOGY.

1. *On the Faculty possessed by certain Elements of the Blood, of regenerating the Vital Properties.* By E. Brown-Séquard, M. D.

At the session of the *Academie des Sciences*, held in October last, Dr. Brown-Séquard detailed a great number of experiments, showing that the contractile and nervous tissues, after having entirely lost their vital properties in consequence of a complete interruption of the circulation of the blood, may recover these properties under the influence of red blood injected into the arteries of these tissues. He remarks:

In a single experiment, it is possible to observe at once almost all the facts above related, as regards the restoration of vital powers in muscles, and in motor and sensitive nerves.

A ligature is put around the aorta, behind the origin of the renal arteries, in a strong rabbit. Voluntary movements, sensibility, reflex actions, the power of motor nerves, and, at last, muscular contractility, successively disappear from the posterior limbs of this animal. I wait until cadaveric rigidity has come,* and even until it exists in all the parts of these limbs, and then I take away the ligature. The anterior part of the body of the rabbit being still living, circulation recommences in the posterior part of the body, and, with the arrival of blood in that part,

* It comes in a very variable time, from one to ten hours after the ligature has been placed on the aorta.

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muscular contractility, the power of the motor nerves, reflex actions, sensibility, and, at last, voluntary movements successively reappear in the posterior limbs. From this experiment, it results that, not only local life, but the properties and actions of full life, can be restored in limbs that have been in the state called rigor mortis, cadaveric, or post mortem rigidity.

Since the 9th of June 1851, when I communicated to the *academie* the details of the first experiments of this kind that I have made, I have performed them many times, and they have been repeated with success by Professor Stannius of Rostock.

Dr. Brown-Séquard has found that the muscular tissues, the motor and sensitive nerves and the spinal cord will recover their vital properties under the influence of red blood injected into their vessels. The contractile fibres around the hair bulbs, which produce the appearance called cutis anserina, may, under this influence, recover their contractility. long after they have lost it. In an experiment on a man guillotined in 1851, the cutis anserina was produced on the fore arm more than twenty-four hours after decapitation, by injecting blood into the brachial artery.

It was found that blood, which had been deprived of its fibrine, would restore the vital properties of these tissues as quickly as that still containing it. This important fact might lead us to doubt the agency of fibrine in the process of nutrition. Experiment also proved that the serum of the blood did not possess this regenerating faculty. Dr. Brown-Séquard concludes that it is principally the oxygen of the blood which restores the vital properties of the tissues, although he does not deny the agency of the blood globules and albumen in aiding the result. He considers that it is a part of the functions of the blood globules to absorb oxygen and carry it to the tissues, and he has, by artificially oxygenating venous blood, produced the phenomena of re-exciting the vital properties of the muscular and nervous tissues.

The contractility of muscles can be reproduced three or four times in the same animal by these injections of oxygenized blood; and the laborious experimenter declares that he has maintained the contractility of a muscle separated from the body of a rabbit for fifteen hours, when fatigue compelled him to stop the experiment. The following are his conclusions:

1. The contractile tissues, i. e. the muscles of animal life, the muscular fibres of the uterus, of the bladder, of the heart, of the bowels, of the iris, of the skin, may, under the influence of red and defibrinated blood, recover their contractility, from half an hour to five hours after having lost it.

2. The motor and sensitive nerves, and the spinal cord, may also, un-

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der the same influence, recover their vital properties, from a quarter of an hour to about an hour after having lost them.

3. The vital properties appear to be independent one from another, and their existence seems to depend upon the organization of the tissues, and upon the maintenance of this organization by red blood.

2. *On the Secretion of the Sugar in the Animal Economy.* By Bernard Henry, M. D.

Dr. Henry, in a late number of the Medical Examiner, has prepared a very accurate and interesting *resumé* of the investigations of the French and German physiologists with regard to the secretion of sugar in the human body. He alludes to the experiments of Lehman, Bernard and Andral on this subject, and calls attention to the attack of M. Figuer upon Bernard's theories, which we have already noticed in our journal, (Vol. V. No. IV.) Dr. Henry considers that we may correctly arrive at the following conclusions:

That sugar is a normal product in man.

That this principle is secreted in the liver, and that it is a normal function of that organ.

That the source of its supply is from nitrogenized elements.

That the food furnishes it also to the system.

That in the glycogenic function there is a sympathy of relation between the liver, the lungs and the cerebral centre.

That in the disease called diabetes mellitus, the equilibrium of the production and destruction is disturbed, and that any one of these three structures may be at fault; and that it is to one or more of them that our remedies must be directed.

That the experiments of Lehmann, Bernard and Andral will warrant the careful allowance of small portions of vegetable food in this disease, and thus relieve our patients from one of the most distressing and trying attendants of the present mode of treatment.

That the labors of the physiologists, and above all, of Mr. Claude Bernard, have paved the way for a better understanding of diabetes mellitus, by demonstrating the condition of the glycogenic function in the state of health; but that close and more extended pathological observations are called for to render his researches available to the physician for a successful plan of treatment of a disease which is rare, but has thus far proved intractable.

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PATHOLOGY.

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1. *Glandular Growths out of or at a Distance from Glands.* By Henry J. Bigelow, M. D.

At a late meeting of the Boston society for medical improvement, Dr. Bigelow made the following remarks, which we find in the Boston Medical and Surgical Journal, for the purpose of taking date in reference to certain observations:

"Formerly, many tumors obtained, indiscriminately, the name "glandular;" subsequently, the term "fibro-plastic" was introduced as designatory of the same class; but finally, many thus called were found really to possess glandular structure. Lebert described certain growths as "hypertrophied glands;" the structure being like that of the secreting glands; botryoidal; irregularly lobulated, though imperfect in ducts. This title does not sufficiently characterize the sort of tumors referred to. Tumors beneath the ear are apt to develop this glandular structure; in the mammary glands, nodules of it may often lie deeply imbedded. In masses thus formed, it is not unnatural to suppose that the new growth might take on the same formation which belongs to the gland, in, or near to, which it is developed. Another, different growth, of a more pervading description, has been styled glandular; the tissue of the gland in which it arises, is apparently infiltrated with the new growth. Dr. B. said that some years since he had reported instances of the former variety of growth being found at a distance from any gland; and, consequently, in no wise associated with glandular structure. He had observed this in the cervical region; and three years ago he removed a tumor from the soft palate, which exhibited a distinctly glandular formation. From its situation we have a direct proof of its independence of glandular tissue in the natural state. A still stronger case is the occurrence of a mass of this glanduliform structure within the diseased shell of a lower jaw bone which Dr. B. had removed. He had, in a third case, removed the same tissue from the antrum.

Dr. Bigelow remarked that he had mentioned his views, incidentally, to Dr. Hastings of California, who told him that M. Robin, with whom he had been lately studying, had conceived the same idea of there being certain extra-glandular growths. Since then, Dr. B. had seen a statement in

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one of the French journals of these views of M. Robin. But he had now to report a still more remarkable phenomenon, viz: true glandular structure occurring in a case of melanotic cancer on the instep.

Within a fortnight a man came to him with a tumor upon the instep; small; about the size of an almond; with two black points upon it. Dr. B. had some suspicion of its being melanotic; on its removal he found it to be unequivocally of that nature; but under the microscope, its structure was also glandular; yet it was, also, cancerous. Recurrence of the disease, after fifteen years, had taken place in this patient, who, at first, had it upon one of his toes, and which was then amputated. It was thought by one of the surgeons who attended him to be "dry gangrene;" by another it was pronounced "cancerous." This same patient has a diseased gland, of the size of a pullet's egg, in the corresponding groin. Dr. Bigelow supposes it to be melanotic.

Dr. Shaw said that M. Robin, in the article alluded to by Dr. Bigelow, had lately reported to the Parisian academy the discovery of extra-glandular tumor eight times in five hundred and fifty cases of promiscuous tumors."

2. *On the Origin of Varicose Veins of the Lower Extremities.* By M. Verneuil. (London Lancet.)

M. Verneuil lately read a paper before the Academy of Medicine of Paris, on the above subject, and strove to make out the following points:

1. Whenever varicose veins which have sprung up spontaneously are observed on the lower extremity, there are deep veins similarly affected in the corresponding part of the same limb.

2. The converse, however, does not hold, for the inter or intra-muscular veins may be found dilated, without any change having taken place in the superficial vessels. But when the deep veins alone are found in an exposed state, it is almost certain that sooner or later the superficial ones will in their turn swell, become tortuous, and very apparent under the skin.

3. The varicose state of the veins of the lower extremity, as it is usually seen, does not primarily arise from the subcutaneous vessels (the internal saphena not excepted,) but generally from the dilatation of the deep veins, and most often from the muscular veins of the calf of the leg! The deep veins are first affected with valvular inefficiency and

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dilatation, and these two lesions then spread to the superaponeurotic branches of the second and third order.

4. This succession of phenomena is not only made manifest by simple dissection, but also by a careful study of the special arrangement of the venous system of the lower extremity.

5. These facts, which may be looked upon as a new discovery, throw much light on the whole subject of varicose veins of the lower extremities. The etiology and symptoms of the affection are thus elucidated, and this circumstance allows of a more rational choice of therapeutical means.

6. The mechanism of relapses will henceforth be more easily understood; for it must be confessed that the obstinate return of the complaint, which experience shows to be so frequent, has been explained more by a train of reasoning than by direct demonstration.

MEDICAL PATHOLOGY AND THERAPEUTICS.

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1. *On the Treatment of Pneumonia with Acetate of Lead.* By M. Burkart.

A writer in *L'Union Medicale*, No. LXXXII, quoting the account given by Dr. Burkart, of the great benefit he has derived from the employment of acetate of lead in pneumonia, states that he can amply confirm it by the results of his own experience of its use, and especially in the aged. In the pneumonia of childhood and the adult, he has always preceded its employment by bleeding, antimony and blisters. But there are cases in which these means either exert no effect, or after a while cease to do so, while in others the patient will not admit of bleeding, or antimony may not be tolerated. In such, lead will be often found a valuable resource, and the pneumonic inflammations that hasten on the progress of softening in phthisis, may be often arrested under its influence. In the aged, defective power contraindicates active bleeding, and the pneumonia, in place of proceeding towards resolution, becomes complicated with abundant secretion, the patient dying from asphyxia, in consequence of his inability to expel this; even if the pneumonia be relieved, the convalescence is tedious and troubled.

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Antimony has less power over these cases, and is ill supported, while the lead gives rise to no dangerous consequence. Still, as a general rule, a local bleeding, and small doses of antimony should be first prescribed; but, if amendment be not very prompt, or debility commences to appear, the lead should be exchanged for the antimony. In other cases, in which the reaction is less at first, and debility of earlier occurrence, the acetate may be given from the beginning. When resolution is very tardy, a large temporary blister will often give the first impulse to a movement that the acetate will continue. The period of convalescence is much diminished, by supplying the patient with a sufficient nourishment when the appetite returns. The mean dose is four or five grains in the twenty-four hours, this being increased, in rare cases only, to six or seven. It may be given in solution in distilled water, with syrup, or in pills, adding opium only when there are indications for it. It must not be omitted for colicky pains and temporary constipation, which are usually relieved by lavements. The author has continued it for three or four weeks without any ill effects being produced, although the characteristic blue color of the gums has been present.

2. *Chloroform Inhalations in Pneumonia.* By Dr. Richter.

Dr. Richter reports in the Berlin *Medecin. Zeitung*, No. XXXII, that Staff Surgeons Drescher and Lemke, after repeated trials, quite confirm the favorable accounts that have been given of the efficacy of chloroform inhalations in the treatment of pneumonia and bronchitis. In their mode of employing it, thirty drops of chloroform are poured upon a piece of closely pressed wadding, a finger or two in breadth, which is wrapped up in another piece of wadding, and held about half an inch from the patient's nose for about five minutes. This is repeated every hour, until some remission of the symptoms occurs, when twenty drops are inhaled every second hour, until convalescence is established. The subjective symptoms—as constriction, pain, irritating cough, are usually diminished, even after the first inhalation, and entirely disappear from the second to the fourth day. At the same time the sputa lose their bloody admixture, remaining tough, however, until about the fourteenth day, when they disappear, as well as the cough. The frequency of the pulse is diminished by the second day, descending, in a case of double pneumonia, from 120 to 80. Sweating generally sets in after the first inhalation, and never later

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than the fourth or fifth. As regards the duration of the disease, complete recovery, so that the patient can be discharged, does not seem to take place more rapidly than under treatment by bleeding. The preferability of this treatment arises from its simplicity of application, its applicability in cases in which from the character of the disease, or the individual, depletory treatment is forbid, and the rapidity with which the distressing symptoms are relieved, and convalescence is commenced—the patient being enabled to leave his bed by the fourteenth day.

3. *Veratrine in Pneumonia.* By MM. Fournier & Aran.

M. Fournier, *L'Union Med.* No. XCIX, furnishes an account of the success M. Aran has obtained from the employment of veratrine. The hyposthenising effects of this drug seem to have been exaggerated; for in the forty-one cases in which he has seen it given, in quantities of from two to three or even four centigrammes, divided into six doses, in the twenty-four hours, no alarming prostration, excessive vomiting, etc. have been observed. Of these patients a fifth part only were females, and the bulk of them were between twenty-four and thirty-six years of age, many being persons of feeble constitution. The majority came in about the fourth or fifth day after the invasion of the disease, and the next day red hepatization was almost always recognized. With the exception of three or four, they all exhibited active febrile reaction. In about one-half the cases, soon after taking the medicine, a deep-seated burning near the spine, and a peculiar sensation at the epigastrium are complained of. Although in a few cases there was no vomiting, this occurred in the others upon an average from three to five times during the first day, tolerance soon taking place, and a peculiar nausea continuing for several days. No active purging resulted from these doses.

The effect on the pulse was constant and sometimes immediate. In twenty-four hours this fell by fifteen to thirty-six beats, and there was the curious phenomenon of an eminently febrile acute affection coexisting with an apyretic condition of the circulation, the pneumonia still pursuing its course, as ascertained by auscultation. The pulse descended to sixty or seventy, or even sometimes to fifty-six or forty-eight, and took several days to reassume its normal frequency, even when the medicine was suspended; it not being uncommon to find it only fifty-five or sixty during the early period of convalescence. Its character was also

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changed, it being less strong and more depressible. That this condition of the pulse was not the result of the emetic power of the drug, was seen in the fact that it prevailed in cases where no vomiting was present, while in others it was not proportionate to the amount of vomiting. The temperature of the body was constantly diminished by 2° or 3°, never, however, descending lower than 96·8°; the skin also remaining constantly moist. A notable languor followed the taking the medicine, being more than a simple sedation, but less than true prostration.

It was, however, not alone upon the febrile condition that the veratrine exerted its power, but also upon the local disease itself, as shown by the changed condition of the respiration, cough, expectoration, and physical signs. In more than two-thirds of these cases the respirations were diminished by four, eight, ten, twelve, fourteen, or even more per minute on the first day—such diminution of the frequency of the respiration being much more in unison with the diminution of the pulmonary lesion than the lessening the number of the pulse was. Whenever a diminished number of the pulse contrasted with a stationary or increased number of respirations, the local disease was of a very menacing character.

Veratrine given alone suffices for the cure of pneumonia; and of twenty-three cases so treated, some of them being severe, twenty-one recovered, resolution usually occurring on the fourth day. The great success, however, that attended bleeding in the rare cases that resisted veratrine alone, induced M. Aran to try the effects in severe cases of commencing treatment by a *single* bleeding. The rapidity of the cure of cases so treated has been remarkable. The veratrine treatment is especially applicable to primary simple pneumonia, occurring in rigorous adults, and is not so well suited for the adynamic forms of the disease.—*Medical Times and Gazette.*

4. *Homœopathy and the Cholera at Marseilles.* By Dr. Bouquet.

Marseilles possesses, it would appear, a very considerable number of homœopathic physicians; to believe them, they would cure all their patients, and it was only in consequence of a kind of blind infatuation that their allopathic brethren refused to adopt their mode of treatment. The authorities, little satisfied with the result of ordinary medicine, wished to know the truth of the matter. They set the homœopaths

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to work, and the following letter, addressed by Dr. Bouquet to the *Gazette des Hôpitaux*, proves once more how easy it is to be deceived as to the efficacy of remedies, until carefully instituted experiments interpose to distinguish the efforts of nature from the action of medicines:

"Homœopathy has just experienced a severe check in our town.

"You have perhaps heard of the noise it made last year with its pretended success in cholera. Dr. Chargé asserted that he had not lost one out of several hundred patients, and he published this statement in the political journals of Lyons and Bordeaux.

"When, during the present year, the scourge visited us anew, the authorities bestirred themselves, and thinking it was their duty to bring the truth to light, they intrusted one of the wards of the Hôtel Dieu to Dr. Chargé. There, assisted by his colleagues in homœopathy, by pharmaciens, and by some young people his adepts, who devoted themselves to tending the patients (for he had found the ordinary staff insufficient and incompetent) he obtained the result which might easily have been anticipated; the broad daylight did not display success.

"Of 26 cholera patients admitted into this ward, 21 died, and M. Chargé withdrew.

"To render the experiment conclusive, a ward had been set apart, in which the patients were treated by rational means, which did not profess to work wonders.

"During the same period, of 25 patients but 11 died.

"Each ward had its turn of reception.

"I think that these facts are sufficiently decisive to render a renewal of such experiments needless, for if science profits by them, which is doubtful, humanity suffers not a little." *Journ. de Méd. et de Chirurg. Prat.* Oct. 1855.

5. *On the use of Setons.* By M. M. Bouvier, Malgaigne, Vespeau, &c. &c.

An interesting debate arose at a recent meeting of the Académie de Médecine of Paris, in which some of the well known names which adorn the French school of medicine participated; and as the subject is one of practical importance, we make an abstract from the Medical Times and Gazette:

This discussion had been caused by the reading of a paper by M. Bouvier on the utility of setons, and on some modifications of the thread and of the instrument to introduce it.

Instead of making use of a bistoury, or of the instrument usually called a seton-needle, he employs a common needle, rendered flat at its point. As to the thread, instead of making use of a twist of cotton, or of a strip of cloth, he employs a single thread, or a very fine cord, plastered with an impervious substance, as wax, able to prevent its becoming altered by pus. To avoid the difficulty of introduc-

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ing the cord, in place of the eye of the needle, there are very small forceps to take the thread.

The advantages of this mode of operating are, that the pain is slight, and that there is no need of changing the thread. In the cases where a single thread does not produce a sufficient irritation, it is easy to put several.

Professor Gerdy does not approve of the employment of setons. He says that they produce not only an inflammation in the neighborhood, but also an irritation at much greater distances. They sometimes increase previously existing congestions or inflammation of the eye, so that they do exactly the reverse of what is expected. Patients frequently attribute to setons the production of these effects, and they complain of dizziness, of a bewildering sensation, and of tinnitus aurium. Blisters also produce sometimes the same effects; so that Professor Gerdy has been led, in certain cases of diseases of the head or of the eyes, to apply small cauteries on the back of the head on the shaved scalp.

M. Bouvier answers, that bad effects resulting from setons, according to Professor Gerdy, exist sometimes, but very rarely.

Professor Malgaigne says that he disagrees completely with his colleagues, and censures the prolonged employment of all kinds of exutories. He doubts there being of any use. Their introduction in the practice of medicine has no rational ground. Setons, cauteries, moxas, may be excellent for eight or ten days, but not more.

Professor Velpeau says that he employs the seton, and highly praises its therapeutical action. As to the novelty of the employment of thread, he says that setons originally were made of a single silk thread. He thinks that the filiform seton of M. Bouvier cannot replace, with advantage, large setons. The therapeutical effects of these last are a revulsion and a suppuration, which are in proportion to the pain, and to the irritation they produce. Professor Velpeau says he has seen the children operated on by M. Bouvier, and he does not believe in the efficacy of setons in these cases. In opposition to Professor Malgaigne, he says that exutories may be very useful, but that they must not be employed indiscriminately. One of them is good in cases where the others would not be. Blisters, for instance, are very useful in acute, superficial opthalmia, and particularly conjunctivitis, while setons in these cases would not be beneficial. Cauteries are employed with advantage in deeply-seated chronic affections. Setons are very useful in cases of amaurosis, which do not depend on an organic disease. He

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says that the dangers of the application of setons, spoken of by Professor Gerdy, are of very rare occurrence. They arise from inflammation of the lymphatics, which vessels are not numerous in the neck. He asserts that abscesses are very rarely produced by setons. He employs setons not only against amaurosis, but also against keratitis becoming chronic, and after the following modes of treatment have failed: bleeding, blistering, purgatives, calomel, etc. Setons are not so useful in vascular keratitis, but they do succeed sometimes.

M. Bouvier answers to Professor Velpeau, that he has not proposed to make use merely of the filiform setons, but also of a gradually increasing number of threads. If serious accidents rendered an energetic and immediate revulsion necessary, he does not say that then a large seton should not be employed. He asserts that he himself was cured of amaurosis by the seton, and that the same was the case with the illustrious Fabricus of Aquapendente. In a case, related by Ruysche, cephalalgia was cured by a seton, and returned after the suppression of this exutory. It was cured a second time by the same means, but reappeared after the cessation of the treatment, and then was cured for a third time by the seton. M. Bouvier insists upon the superiority of setons to blisters.

M. Desportes says that he proposed long ago to employ filiform setons, with openings and many threads, so as to produce a greater revulsion.

M. Clot-Bey, who usually resides in Egypt, says that the Arabs employ small setons, placed near the eyes, and sometimes on the eye-lids themselves, to cure diseases of the eyes. He has often witnessed the success of this mode of treatment.

Another discussion has taken place at the Académie de Médecine, mostly between Professors Piorry and Bouillaud, the first stating that it would be very important to study auscultation on dead bodies, the second asserting that very little could be learned by such means.

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OBSTETRICS.

*Illustrations of the Influence of Pregnancy in controlling or retarding the development of certain Diseases.** By W. F. Montgomery, M. D. Professor of Midwifery to the King and Queen's College of Physicians.

On a former occasion, when treating of the condition of pregnancy, I took occasion to remark that it appeared from experience, that women who bear children generally enjoy more even health and are less disposed to disease than those who lead a life of celibacy, or who, having married, remain unfruitful; so that Gardien seems to express no more than the truth when he says, "Dès qu'une femme est grosse les probabilités de sa vie augmentent;" and this is what we ought, *a priori*, to expect, because, childbearing being the ordinance of an allwise Providence, we should anticipate that the fulfillment of the duty thus ordained would conduce to the welfare of those on whom it has been devolved.

It seems in conformity with such a view to believe, what indeed, I think, experience has taught us, that pregnancy acts in a great degree as a protection against the reception of disease, and perhaps on the common principle, that during the continuance of one very active operation in the system, it is thereby rendered less liable to be invaded or acted on by another; thus it has been observed, that during epidemics of contagious diseases of different kinds, a much smaller proportion of pregnant women have been attacked than of others: but when attacked, they suffer severely; thus, when the cholera visited this country, the proportion of pregnant women who took the disease was very small, but all who caught it died, I believe, without almost a single exception. Gardien's experience led him to a similar conclusion, he says: "Les femmes enceintes sont moins exposées a gagner les maladies contagieuses; mais lorsqu' elles en sont atteintes, elles succombent plus promptement."†

I think also I have seen sufficient to satisfy me that pregnancy does, at least occasionally, exercise another kind of influence over disease in the system, namely, of preventing its development during that state, although the infection may have been caught; as is proved by the disease showing itself immediately after delivery, as in the following cases:

Mrs. W. when in the ninth month of pregnancy, was much about her brother, who was dangerously ill of malignant scarlatina; she seemed to have escaped the danger completely, but the day after her delivery she was covered with the disease, of which she died in a few days; between the time of her exposure to the infection and her delivery, there had intervened three weeks, during which she appeared to be quite well.

When Mrs. F. was in the eighth month of pregnancy, her husband had typhus fever, in which she assiduously attended him; after his recovery, she went to her father's house, some fifty miles from town, where she was delivered in due time, and immediately afterwards was

* Read before the Association of the College of Physicians of Ireland.
† Traité d'Accouchement, tom. ii, p. 30.

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seized with typhus fever, of which she died in eight days; between five and six weeks had elapsed between Mr. F.'s illness and her labor, and during that interval, she seemed in perfect health.

In the month of November 1854, I attended a young lady in her first confinement; previous to which she had both the lower extremities much enlarged by anasarca, but she appeared, in other respects, quite well, with one exception, which was that she had such *soreness* of the abdomen, she found a difficulty in lying on either side: and when I passed my hand over the abdomen, she complained that the pressure hurt her everywhere.

On the 12th she was confined, after a favorable labor, but the abdominal tenderness remained, and there was a peculiar doughy feel of the whole abdomen; next day this was equally felt, but with little or no pain or fever, and a perfectly quiet pulse.

On the 14th I found the insteps of both feet, but particularly the left, covered with well-developed erysipelas; her mother, who seemed very anxious about her, was present when I examined the feet, and on our reaching the drawing room said, "Doctor, isn't that very like erysipelas?" I said, "Yes, certainly, there was no doubt about it."—"Dear me, sir, do you think she could have taken it from her husband." She then, for the first time, informed me, that some weeks before leaving home, to come to town for her confinement, her husband had a severe attack of erysipelas, during which she had assiduously nurse-tended him. Immediately on the appearance of the erysipelas on the feet, the abdominal symptoms began to decline, and after two or three days, ceased to exist. I cannot but believe that this lady caught the infection from her husband during her close attendance on him, that it remained in abeyance until gestation was over, and was then developed. She recovered well.

It is, I believe, a matter of common observation, that when women who have been laboring under certain forms of disease happen to conceive, the morbid affection previously existing is oftentimes either greatly mitigated, checked, or even altogether suspended for a time, as has been frequently observed in persons affected with phthisis; though I must add, that the influence of pregnancy in cases of phthisis is a question on which a variety of discordant opinions has been given by high authorities. Andral's* conclusion, from his latest observations, is, "that in the great majority of cases the symptoms of phthisis are suspended, or at least remain stationary during the course of pregnancy." Louis† says he is not "in a condition to determine whether pregnancy is, or is not capable of retarding the progress of phthisis," but he suggests that the fact might be, that several of the symptoms become somewhat more obscure during pregnancy, without any check being in reality given to the advance of the disease. My own experience would lead me to the conclusion, that if a woman predisposed to ththisis, but in whom the disease has not actually become developed, prove pregnant, she is likely to be benefited thereby; and I think I have seen life thus prolonged, for years, in several instances; but, on the other hand, if pregnancy takes place in a woman already actually in consumption, or if this disease supervene on pregnancy, the fatal issue is as likely to be accelerated as postponed, or, perhaps, even more so.

Several years since, I had a patient under my care affected with white swelling on the elbow-joint, which had gone to a great length, and was

* Clinque Médicale, tom. iv. p. 367

† Researches on Phthisis, Walshe's Translation, pp. 279, 280.

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very little benefited by treatment, when all of a sudden a very rapid amendment was observed. On questioning the lady, I found that she had reason to think herself about six weeks pregnant, which was the fact; from that time, the cure advanced uninterruptedly, so that before the end of her gestation the arm was perfectly well, and has continued so ever since, she having, in the interval, borne several children.

Dublin Med. Jour.

TOXICOLOGY.

On some singular Nervous Affections due to a slow Poisoning by Nicotine inhaled during Smoking.

Siebert describes, in his Memoir on the Diagnosis of Diseases of the Abdomen, a work which has just issued from the press, the pernicious effects of the use of the cigar. These effects depend on a slow poisoning by nicotine, and constitute serious affections of long duration, the causes and origin of which usually escape the notice of the practitioner, and determine the failure of the various modes of treatment employed.

Berzelius had already shown that cigars contained more nicotine than ordinary cut tobacco. In consequence of the mode of preparation which tobacco cut up for the pipe undergoes, the nicotine in it is already reduced to its minimum of activity. But this is not the case with the leaves destined to be converted into cigars; the latter, in fact, are not subjected to the same preliminary preparation. Accordingly, it is chiefly in consequence of the use of cigars that the affection to which we are directing attention is observed, and which consists in a very obstinate neurosis, slowly developed, under the influence of the continued ingestion of very minute quantities of nicotine. This neurosis takes the form of an irritation of the spine, and produces, according to the point of the spinal marrow principally affected, different eccentric phenomena, such as bronchial spasms, a feeling of suffocation, palpitation of the heart, gastrodynia and vomiting, mesenteric neuralgia, &c. As examples of this slow intoxication, we shall cite the following cases reported by Siebert:

A very robust man, aged 30, was from 1840 to 1845 liable to nervous attacks of the most varied kind, such as numbness of the forearms and hands, palpitations, hypogastric op-

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pression, want of appetite, and even vomitings; spasms of the sphincter ani, and nocturnal seminal emissions, followed by great depression. The motor power of the lower extremities was from time to time disturbed; deambulation, as well as ascending a height, became uncertain; vertigo and double vision were superadded. These symptoms, at first attributed by Siebert to hyperemia of the spinal marrow, were combated by cupping, leeching and purgation, but they only got worse. Preparations of iron always gave relief, but without producing a complete cure. Siebert then insisted that his patient, who was in the habit of smoking a number of cigars of good quality, should abstain from them for at least four weeks. At the end of this time he felt himself quite another man. To complete his cure, he repaired to the chalybeate waters of Steben, which strengthened him much, and from which he returned completely restored. During the winter of 1845, he was tempted to smoke a cigar, when the symptoms above described immediately returned, and only yielded to the use of iron, continued for four weeks.

The second case was that of a man of cachectic and almost mummified aspect, who consulted Siebert for a total loss of appetite. The only time at which his state was at all supportable was after smoking a very strong cigar. He was attacked regularly every afternoon with violent colicky pains, which lasted several hours. Purgatives were tried without success. For years he had tremors and weakness in the extremities, palpitations of the heart, and sometimes vomiting; for some months past he had, in addition, complained of a disagreeable sensation, like a blast, in the lower part of the spinal column, combined with intestinal pains recurring daily, commencing each time in the umbilical region, spreading through the entire abdomen, and becoming concentrated in the back; these pains were intolerable, rending, crushing, but ceased during the night. He was not subject to vomitings at other times, nor was there any derangement of the alvine evacuations. From the time that he gave up the use of cigars the symptoms entirely ceased; but not being able to deny himself the enjoyment of smoking beyond four weeks, he resumed the habit, and the pains returned.

It is not every kind of cigar that produces these effects—those of a fine and narcotic description are generally the most injurious. Neither are all smokers subject to the poisonous effects. We may, however, assert that nervous affections occur more frequently in men, since the cigar has displaced the pipe. It is often very difficult to ascertain the

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causes and origin of the disease, for enthusiastic smokers deceive both themselves and their physicians; and, what is worse, the symptoms produced by poisoning with nicotine are sometimes quickly dissipated by the use of the cigar, just as those due to opium often yield to a fresh dose of the narcotic.

Strong cigars at first excite the appetite, and after meals favor stomachic digestion, which circumstance induces the true smoker to fly to his cigar immediately he has swallowed the last morsel. These individuals ought carefully to avoid any derangement of digestion. The appetite and the digestive powers soon diminish, enteric pains become developed, and at the end of a short time constitute true neuralgic paroxysms.

The *Allgemeine Medicinische Central Zeitung*, from which this article is extracted, adds the following note:—"Our colleague, Dr. Ravoth, has verbally informed us, that he has seen a case of intoxication by nicotine in a physician already advanced in life, who smoked much, but the pipe." *Journal de Médecine de Bruxelles*, and *Revue Médico-Chirurgicale de Paris*, July 1855, p. 31.

[We meet very commonly with a similar train of nervous symptoms amongst the lower classes of this country, many of whom smoke common tobacco to excess. And our lamented friend, the late Dr. Graves, was so severely affected with vertigo and other nervous symptoms from smoking a cigar, that during the last four or five years of his life, he was compelled to abandon the practice altogether.

Editor Dublin Quarterly Journal.]

THERAPEUTICAL RECORD.

Aneurism of the superior palatine artery.—By M. THIERLINCK.—This surgical curiosity was met with in the case of a man, æt. 74. The tumor occupied the roof of the palate, which bled so frequently that the patient was much exhausted. The tumor was soft, elastic, and pulsated synchronously with the heart, alternately expanding and diminishing. Its cause was unknown, and it had lasted for three weeks. The actual cautery was employed, the slough separated in eight days, the hemorrhage did not recur, and a perfect cure resulted.—(*Dublin Hospital Gaz.* from *Gazette Medicale.*) *Boston Med. and Surg. Journal.*

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Cheiloplasty.—Dr. Eve (Nashville Journal of Medicine) reports an operation for the restoration of the entire upper lip, and also a wing of the nose and the base of the column attached to the vomer, which has terminated successfully. This is one of the most extensive operations of the kind recorded. In Dr. Carnochan's case, reported in the American Medical Monthly, January 1854, the loss of substance was confined to the upper lip, and there was enough mucous membrane to cover the newly made one. In Dr. Eve's case there was no mucous membrane remaining, to cover the artificial lip, and in addition he had to make a wing to the nose.

Chronic entropium.—Mr. Wm. Butten reports, in a late number of the Lancet, two obstinate cases of entropium, both of which had resisted a great variety of treatment, but which were cured by the application of collodion to the skin of the eyelid, previously corrugated by the thumb and finger. Several layers are successively applied and allowed to dry before the fingers are removed. The application is made at first every other day, and afterwards at longer intervals.

Cider in gout.—The value of cider as a prophylactic of cholera, has been discussed in our pages, and we notice that a correspondent of the Medical Times and Gazette suggests the habitual use of cider and its cognate beverage, perry, by individuals of a gouty diathesis, believing that the malic acid, like the citric, would aid in eliminating the free lithic acid from the system. He thus writes:

> I am certain that many of the old fashioned ideas on the subject of drinks for the gouty, are susceptible of improvement. My experience coincides with that of those physicians who recommend to their gouty patients the cautious use of Rhenish and subacid French wines, especially the white varieties, (Sauterne, Chablis, &c.) If the stomach can manage them at the outset, I am of opinion that they are much more suitable than the Spanish wines, in the process of secondary assimilation. I think the virtues of (so called) "Sherry" are very mythical in these cases.

Diarrhœa.—The root of the blackberry (rubus villosus) which has generally been regarded as a simple astringent, is declared by Dr. Snead, (Southern Medical and Surgical Journal,) to possess most valuable tonic properties, and he recommends it as a warm astringent tonic in chronic diarrhœas and dysenteries. The fresh root should be grated, and a cold infusion obtained.

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Hemorrhage in fevers.—Dr. Kennedy, (Dublin Quarterly,) after analyzing 262 cases of fever in which hemorrhage occurred, declares his belief in the beneficial agency of this process, and is disposed to doubt the propriety of abandoning the use of blood letting so completely, as the profession has done of late. Inasmuch as these large discharges of blood so often moderate the fever and cure the disease, it may be fair to infer that the judicious abstraction of blood would frequently produce the same favorable result.

Hemorrhoids.—Dr. Buckingham (Boston Medical and Surgical Journal,) prescribes the pickled unripe pepper as an article of diet for persons who suffer with hemorrhoids. He has often found this treatment successful. His dose is one pepper daily, to be eaten at dinner.

Hunter's doctrines of inflammation questioned before the Academy of Medicine of Paris.—M. Paul Broca, deputy professor at the faculty of medicine, has lately read a paper before the academy, in which he strives to show that such pathological phenomena as adhesion, ulceration and gangrene, may take place without any inflammation; and that, carried away by Hunter's views, we allow phlogosis to play a much more important part than is really the case. The author grounds his belief mainly on the fact that *nonvascular* textures, like the cornea, cartilages and ligaments, undergo certain changes, without the agency of inflammation, and attempts to show that the latter is, in ulceration and gangrene, more an effect than a cause. The paper is unfinished, as the attack upon adhesive inflammation is to be subsequently made; but we are bound to say that, so far as it has gone, it is full of sound views, good reasoning, and conclusive facts.—*Lancet.*

Hydrophobia in France.—From an enquiry instituted by the French government, it appears that in 1852 no less than 48 cases of hydrophobia occurred in France, these being distributed over fourteen departments. Returns are now ordered to be made every year; and one consequence of drawing attention to the subject has been the inundation of the government with infallible specifics. These have been handed over to the *Académie de Médicine* for examination, and M. Bouchardat, in a recent report, exposes their ridiculous pretensions. He observes that there is not one of these recipes which has not at some former epoch been brought

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forward, and allowed to fall into deserved oblivion. The authors of these communications must also be edified at learning that the *arcana*, which they flattered themselves they alone possessed, reach the government by different channels, and are, indeed, traceable to quite the infancy of art.—*Med. Times and Gaz.* 1855.

Lithectasy.—Professor Erichsen of the University college hospital (Times and Gazette) has taken a calculus from the bladder of a little girl aged three years and a half, by a process of dilatation, which is rapid and successful. He used sponge tents for three or four hours before the attempt to extract. Then putting the child under the influence of chloroform, the sponges are withdrawn, and a bivalve dilator introduced for a few minutes. A pair of small forceps were then passed into the bladder, and the stone, as large as a shilling piece, was easily extracted. No incision or laceration of the meatus was necessary.

Porcelain bougies.—Dr. Moseley (London Lancet) brings to the notice of the profession a method of treating anal strictures, by introducing bougies of finely glazed earthenware, which are made solid and of graduated sizes. They pass, when smeared with a liniment of olive oil and lard, with great ease, and are much more comfortable than silver or gutta percha bougies, which may be probably owing to their extreme smoothness. Dr. Moseley has used these instruments in anal strictures and in those cases of constipation dependent upon a narrowing of the rectum; and he has also obtained great relief from their introduction in irritable and chronic conditions of the rectum. They should be, under these circumstances, artificially cooled and well smeared with an anodyne ointment.

Uterine hæmorrhage.—Dr. Landman, (*Nederlandsch Lancet,*) referring to the well known sympathy between the breasts and the uterus, reports two cases of obstinate hæmorrhage of the womb, which were checked by actively exciting the breasts by suction. The case with whom uterine flooding had invariably occurred after delivery, was promptly relieved by the application of the lips of a woman to the nipple, when the uterus at once contracted and the hæmorrhage ceased.

Vaccination in abdominal typhus.—Dr. De Gressot has communicated to the Academy of Medicine some remarks upon

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the probable consequences of the connection established by some medical men between small-pox and typhus fever. He asks if, admitting the connection to exist, it is not desirable to attempt the prevention of the eruption on the intestinal mucous membrane by vaccination, performed upon some accessible point of its surface, in the same manner as the cutaneous eruption is combatted by vaccination practiced on the skin.—*New Orleans Hosp. Gazette.*

BIBLIOGRAPHICAL RECORD.

I. *Yellow Fever considered in its historical, pathological and therapeutical Relations, including a Sketch of the Disease as it has occurred in Philadelphia from* 1699 *to* 1854—*with an examination of the connections between it and the Fevers known under the same name in other parts of temperate as well as tropical Regions.* By R. La Roche, M. D. Member of the American Philosophical Society, of the American Medical Association, Fellow of the College of Physicians of Philadelphia, Corresponding Member of the Imperial Academy of Medicine, and Foreign Associate of the Medical Society of Emulation of Paris, of the Academies of Sciences of Turin, Copenhagen, Stockholm, Nancy and New Orleans; of the Medical Societies of Naples, Marseilles, Lyons, &c. Philadelphia. Blanchard & Lea. 1855. (Received from A. Morris.)

We have read in the strong and earnest language of De Foe, of the London Plague, and we remember with delight how Boccacio has wreathed the ghastly corpse with the beauties and graces of the Decameron. Even in later times and in our own country, history records the ravages of many fierce epidemics, and our fathers and grand-fathers have told the horrid story of the yellow fever years of Philadelphia and New York, whilst this generation has seen three epidemics of Asiatic cholera pass over our country with resistless power, but nothing has ever yet stirred the depths of the nation's heart like the yellow fever epidemics of the last three years.

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New Orleans in 1853, was first the scene of this unprecedented slaughter. Yellow fever had been there often before, indeed seemed almost to have become a permanent resident of that unfortunate city; but now, it bursts all of its ancient bounds—it flies everywhere—up the rivers and bayous, along the lines of railroads, and on the track of the steamers. The people all sicken, and those who are spared the fever, starve for want of food, until the *American people* and the *Howard association* come to their rescue. The following year only told the same story, and Savannah became the scene of its awful ravages, whilst the calamities of Norfolk and Portsmouth are still vividly impressed on the memories of all.

Whilst these sad visitations have roused the attention of the whole people to an extent unparalleled in our history, and excited the medical profession to almost superhuman efforts to resist its ravages, the great work on yellow fever, which has been in preparation for years past, from the pen of one of our most laborious and eminent physicians, is announced. No book ever issued from the press at a more opportune moment, and all interested in the great questions involved in the consideration of the laws which govern yellow fever, have eagerly sought its perusal.

This work of Dr. La Roche now lies on our table; and as we survey the two ponderous volumes containing fifteen hundred pages of small print; when we glance at the list of authorities referred to, filling sixty-one pages, and including more than one thousand different authors, we almost doubt the capacity of our modest monthly to digest such a gigantic repast; and our readers must therefore pardon us if we confine our attention to some particular points which we regard as deserving our special notice; for to give to this enormous production a complete and thorough examination would be impossible; and unless there is a medical journal in Brobdinag, we doubt whether it will ever be attempted.

We accord to the author of the book before us the well deserved tribute of our admiration and respect for the amount

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of labor and research he has displayed in its preparation. It is indeed an encyclopedia of the literature of yellow fever, and on almost all points he exhausts the subject. No opportunity has been neglected by which information could be obtained; no work on the subject entirely overlooked, and we arise from an examination of the whole with the words on our lips, *teres totus, at que rotundus.*

Whilst we premise the remarks we intend to make on certain portions of this treatise of Dr. La Roche, by the expression of our praise, and whilst we take the occasion to declare our great respect for its author, than whom our country does not contain a more accomplished scholar, yet a careful examination of those parts which are now of the most paramount importance to the public weal has convinced us that it would not be safe to permit this elaborate production to be regarded as the undisputed authority on all of the questions involved, which, from its size, completeness of finish and great extent of research, we would otherwise be inclined to accord to it.

The great question to be determined with regard to yellow fever, is not an examination of its symptoms; unfortunately they are generally too manifest to require much delicacy of diagnosis—nor are we yet to expect any suggestion which will enable us to treat the malignant disease with success. All modes of treatment alike fail. We know not how to cure it. The only hope we can have is to find out how to avoid the generation of the disease. The ounce of prevention may save us—there is no pound of cure.

To determine this point, we must find out the cause of yellow fever. We must ascertain whether there may not be many causes. Then we should study all the laws which promote the development of the originating influence. And lastly, we should learn how to prevent its formation.

The author who attempts the settlement of this question, and who succeeds, will stand on the pyramid of fame side by side with Jenner. To prevent disease is assuredly a greater triumph than to cure it; and he who saves our race

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from all the pains and suffering of prolonged disease, is a greater benefactor than if he brought with him to the bedside of the invalid a sovereign specific.

There is but one way to arrive at truth on the subject under discussion. We must begin our investigations by swearing to preserve the most rigid and uncompromising impartiality—to gather all the facts, come how they may—and when they are marshaled together by the unbiassed and independent collector, then follow them wherever they may lead. This is an immense labor; but it is only thus that we can attain the object. Sift every fact (so called) to its foundation, but give every statement its legitimate weight. When one step is made, stand still until the hatchet of truth has cut another for us, and in spite of the slippery glaciers of theory or the confusions of cloudy and incorrect statements, we will at last arrive on the summit.

Without entering into detail, for we need not search this voluminous work to collect the evidence, we broadly state that Dr. La Roche is not an impartial student of the etiology of yellow fever. He shows that he is the slave of a theory, and the follower of a certain creed throughout the whole discussion of this important subject. He honestly, we have no doubt, labors to be impartial. He makes a great parade of evidence on both sides of the question, but he sets up his nine-pins to be knocked down by the first ball, and sends with the bane of the opposing theory, the wholesome antidote of his own favorite dogma.

There are three modes by which yellow fever is supposed to originate. By contagion, like small-pox; from local causes, as gaol fever or ship fever, or malarious fevers; and by importation of the materies morbi by means of vessels, clothes, or other modes of transmission. These three schools are positively distinct. The contagionist believes that the poison is conveyed from one person to another; the believer in local origin contends that there is some resident cause; whilst the importationist believes that some poison, whether a gas or an insect, which will produce the disease, is carried

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from place to place, and the group of symptoms called yellow fever is the result. There are modifications of these three theories—one running into the other—but all reducible to the three. One thinks that the disease is of local origin, but contagious also; some declare that the contagion is brought by sailors, whilst the vessel bears within it the materies morbi. But if we settle which of the three produces the disease, we will know how to prevent it. If the cause is local, we must look to the condition of each point where it appears; if it is contagious, we must put the sick into isolated hospitals; if it is imported in vessels, we must quarantine every vessel from a yellow fever port, and prevent the deposit of the fatal germ.

It is only necessary to turn over the leaves of the book under review, to see that Dr. La Roche is a believer in the local origin of yellow fever, and his right to be regarded as an impartial historian is at once rendered questionable, whilst his mode of presenting the arguments of his party and his careful emphasis of the strong points of his own theory are very apparent. The book thus loses its chief value; and however useful it may be as a collection of much of the literature of yellow fever, yet the great question, which overrides all others, is no nearer a settlement than before he wrote it; and it remains for some more stoic pen to resist the fascinations of theory, and be guided by the facts wherever they may lead.

We object to the method of discussing the causes of yellow fever, adopted by Dr. La Roche, because he does not give each of these theories a fair hearing. He believes in the local origin of the disease, and he wants to make his reader agree with him. His great difficulty is to explain away all the instances where yellow fever has seemed to have been brought in vessels coming from some infected port; to which may be added the fact that it is a disease of the sea coast, or always was until steamboats or railroad cars might be supposed to transport the germ into the more inland regions. This is now the hardest knot to untie,

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whilst the proofs of the contagiousness of the disease, once almost universally believed by the profession, have been so frequently contradicted by the experience of our own times, as to be easily defeated. Everybody knows that yellow fever patients have been scattered through the surrounding country, and yet no one has caught the disease; but the fact that vessels are so often accused of having originated the disease, and the suspicious circumstances attending these rumors, are much more difficult to overcome. Our author dexterously picks out the weaker antagonist in the following language:

Deferring to a future chapter what may be said relative to the theory of the transmissibility of the disease without the aid of a contagious poison, it may be remarked, that from what precedes we may infer that the question as to the origin and propagation of the yellow fever here and elsewhere, resolves itself, in great measure, into that of the contagious or noncontagious nature of the fever; for, if the latter is imported, *per se*, through the instrumentality of individuals laboring under or convalesing from it, it must be endowed with contagious properties.

In other words, contagion and importation are one and the same thing. If the disease is brought in a vessel, it must be brought by passengers; this is the doctrine of Dr. La Roche. Why this assumption? Has a vessel nothing on board but its crew or passengers? Why may not the poison be in the cargo; or in the water, or air, or clothes, or rigging or ballast? But this assumption is necessary to put our author on vantage ground, and thus he says:

The point at issue being thus narrowed to the question of the contagious or noncontagious character of the fever, we may proceed to the examination of that question with a greater chance of avoiding repetitions and confusion, and without the necessity of examining separately the statements afloat respecting the introduction of the disease from abroad—those statements finding an appropriate place among the facts by which the doctrine of contagion may be supported, while their confutation—if confuted they can be—will be used as evidence of the fallaciousness of that doctrine.

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Importation and contagion being the same thing, I'll kill contagion with a thousand facts, and importation is overwhelmed in its ruins.

The preliminary arrangement by which the troublesome customer is avoided being completed, our author proceeds scientifically to demolish the contagionist, and he does it most effectually; but he never separates the importation doctrine from its more vulnerable adjunct. Such expressions as these constantly occur: "By those who advocate the *doctrine* of contagion and importation." Page 226, vol. 2. "The *opinion* of the contagious character and transmissibility of the yellow fever poison." Page 235. "Those who regard the disease as of exotic origin, endowed with contagious properties, *and* capable of being carried from place to place by ships or otherwise." Page 245.

On the other hand, he carefully identifies the theory of the domestic origin of the disease with an opposition to contagion, being an equally unwarrantable assumption. Why should the disease be noncontagious because it has a local origin? Even Dr. Rush, the father of the local origin school, is acknowledged (page 241) to have still regarded it as a contagious disease, and yet, throughout the whole of this discussion, we see all the arguments bearing down irresistibly on the idea of contagion, which being disproved, settles every other question.

One very effective argument in favor of the local origin and noncontagion school, and against the contagionist and importationist doctrine, imitating the extraordinary divisions of the question employed by our author, is the fact that the number of the noncontagionists are very large, yet we know that at one period the *contagionists* included the whole profession. For instance, in 1793, says our author:

The experience of that eventful season, combined with the result of observations collected here in subsequent years, and in other places, has taught us to connect the idea of the development of the yellow fever with the existence of atmospheric peculiarities and conditions of localities, such as

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were noted at the time. But, at the period in question, experience had led to no knowledge of the kind; and, to the few who bore in mind that epidemics of yellow fever had existed in former days, *it never occurred* that the disease might be due to such causes. Hence, what would now be considered of ominous import, was overlooked by the physicians and public of the day.

The same general belief as to the importation of the disease is proven by this extract:

Mr. Chew, in a passage I insert, not as expressing his individual sentiments on the subject, but that of the public generally and of the medical practitioners of the day, says: "I well remember it was the universal opinion that the disease was imported in a vessel from some part of the West Indies, and was communicated by the clothing contained in the chest of a person who died of the disease in the West Indies, (Willing says Barbadoes;) and that the person or persons who were present at the opening of the said chest were the first who were taken with the yellow fever; and to some or all of them it had proved fatal. I also perfectly remember that Drs. Th. Bond and Phineas Bond, Dr. Graham and Dr. Cadwalader, with whom I was particularly acquainted, were all of them of opinion that it was imported in the manner above stated.

The whole of the story is just this: Everybody in 1747 believed in the importation of the disease; everybody in 1793 thought it contagious; and everybody in 1820 believed it to be of local origin; and they all arrive at this conclusion just as our author does, by holding an opinion, and making everything chime in with the preconceived theory.

Another argument used is to count the number on each side of the question, and see which is the most numerous. Drs. A, B, C and D are contagionists and importationists, (synonimous terms,) Drs. I to X are noncontagionists and local originists. There are more men on my side than yours. What is this citation of authorities worth? Newton's apple or Franklin's kite could upset a thousand windy philosophers and learned authors. If our author had published the history of the yellow fever epidemic of Boa Vista, as described

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by Dr. McWilliams, the advocates of the importation theory might have considered that their side of the question had been fairly set forth, and the ingenious and learned gentleman would have found it difficult to deny the almost irresistible conclusion that in that instance the disease was brought to the ill fated place by the Eclair. We, however, nowhere see this important report accurately described, and here select the following passages from the work before us, to show how cautiously the whole subject is avoided.

The question of the introduction of the fever into Boa Vista, which has given rise to an animated controversy, is foreign to the object of the present enquiry, and must, therefore, be passed by unnoticed. Vol. ii, p. 444.

Immediately following this paragraph, wherein the author declines to give Dr. McWilliams' powerful facts in favor of the importation doctrine, we have three pages devoted to an argument drawn from the report of Dr. King on the same subject, who, being a believer in the local origin of the disease, is permitted to have free access to these pages. The same course is again pursued in page 491:

On the subject of the more recent case of Boa Vista, on which so much reliance has been placed to prove the transmissibility of the yellow fever from the original focus of infection to other localities more or less distant, and which has caused the conversion of some writers,* and is regarded by others as alone sufficient to establish beyond the possibility of doubt the contagious nature of the disease, I cannot and need not enter at large.

In the paragraphs following this passage, although Dr. McWilliams is not permitted to speak for himself, yet we have a carefully prepared argument to throw suspicion on his statements. And lastly, at page 804 of the Appendix, "the reader, curious in such matters, is referred to the report and late work of Dr. King, on the subject of the fever in Boa Vista"—the said King being the advocate of local origin, and hence on the right side of the question.

* British and For. Med. Chir. Rev. vol. i. No. 1.

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Contagionists not particular in the Choice of Facts is the heading of one of the arguments against the opponents of our author's theory. A rule ought always to work both ways; and if the exhibition we have just given of the careful avoidance of the important case of the Eclair at Boa Vista is not enough to show that this charge may be returned upon our author, we will give another instance, which will be satisfactory. Nothing embarrasses a decided contagionist, (p. 264,) says Dr. La Roche, true to his theory; and yet whilst he includes Dr. Archer of Norfolk in his list of noncontagionists, and of course as an advocate of the local origin of the epidemic of 1821, he forgets to publish that gentleman's report, which contains the following passages:

On the 20th of July a vessel from Point Peter, Guadaloupe, laden with rum, sugar and molasses, arrived in the harbor. Having discharged her cargo at an upper wharf, her bilge water was pumped out on the dock, between Southgate's and Warren's wharves, which was found to be so putrid and offensive, as to render it expedient that the doors and windows of a neighboring house should be closed, in order to exclude the effluvia arising from it. This and Southgate's ware-house were about equally distant from the vessel—say 15 or 20 yards—one on the east, the other on the west side of the dock.

On the 1st of August Mr. Price, acting as clerk in the ware-house, was taken sick with fever, strongly marked with symptoms of malignancy, and died on the ——— On the same day a negro woman, cook to the family occupying the other house, was attacked, and died on the 9th. On the 4th two ladies of the family sickened, the elder of whom died on the 10th. About the same time a boy, aged 16, and an infant in the same family were attacked, but both recovered.

On the 9th the lady of the house had a slight attack, from which she soon recovered, and the only one who escaped of the whole family, seven in number, was the master of the house, whose duties fortunately took him from home at the time the bilge water was discharged. Young Piercy, who had assisted in pumping the vessel, and a boy by the name of Andrews, who had frequently been about her at the time, both sickened and died, one about the 15th day of the disease, the other within 48 hours after the attack.

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And again—Dr. Archer, who was writing at the time when the believers in local origin were largely in the ascendant, yet could not avoid acknowledging the influence of the Guadaloupe vessel in originating the disease, remarks:

I do not wish to be understood as subscribing to the doctrine of contagion and importation of disease, as generally implied, when I say that the vessel from Guadaloupe had some agency in its production in the present instance. A foul air generated on shipboard, and one brought from a foreign port, are two distinct things; but that bilge water, vegetable, and, perhaps, animal matter and the like, whether of foreign or local origin, under certain circumstances of atmosphere, temperature, &c. are capable of engendering disease, no one, I am persuaded, is prepared to deny. For this reason our quarantine restrictions, which are absurdly severe upon passengers and crews (as in no instance that has come within my knowledge has the yellow fever ever been communicated from the patient to the attendant,) should be more strictly and scrupulously enforced with respect to vessels and cargoes.

Our author excludes these forcible passages of Dr. Archer's report from his readers, and yet at page 379 he quotes the same paper to show that there was a filthy dock and other favorable circumstances, to account for the spontaneous origin of the fever.

When we see the special pleading employed with regard to the Boa Vista case and Dr. Archer's report, which happen to be familiar, we not unnaturally suspect that the same course might have been pursued with regard to the many other quotations scattered through the book. "Nothing embarrasses a decided (non) contagionist."

After having devoted *thirty* pages of his work to the proofs of contagion and importation, which might have been profitably extended by including the history of the Eclair at Boa Vista and the Bann at Ascension, Dr. La Roche proceeds, in *three hundred and thirty pages*, to give the opponents of his creed *all* the facts opposed to their contagion notions, and yet he is not quite satisfied. He fears that the importa-

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tionists may still have a spark of life remaining in spite of their forced alliance with their unfortunate brethren of the contagion school, so we have an Appendix of forty pages devoted to their entire annihilation. Some of the facts in favor of the disease having been brought to various seaports by vessels coming from yellow fever ports, are "still held up by the advocates of contagion," whilst all the evidence "has been and continues to be scraped up to prove its exotic origin." So he proceeds to administer the *coup de grace* to his enemies, and draws his arguments principally from the epidemics of Philadelphia, which have come under his own observation. The most recent of these epidemics occurred in 1853; and as the obstinate importationists insisted that the disease in that instance was imported, he proceeds to give his account of the affair, which we will condense as carefully as possible.

The bark Mandarin left Cienfuegos, a port of Cuba, for Philadelphia on the 25th of June 1853. There had been no yellow fever in that city for *thirty-three years*, "save a few isolated cases in 1826."

She arrived at the lazaretto the 12th of July, and had lost two men with *fever* during her passage—the first dying on the fourth day, and the second on the fifth day of his sickness. At the lazaretto the vessel was thoroughly cleaned, fumigated and ventilated. The bedding and clothing of the deceased sailors were destroyed, and all the clothing washed, whilst the vessel was whitewashed and fumigated with chloride of lime thoroughly. "*She was detained an entire day!*" The crew were in good health.

On the 13th of July the Mandarin went to South street wharf, and lay there three days. Then she dropped to the lower side of the first pier below Lombard street, and discharged her cargo. On the 20th she removed to the first pier above Almond street, and remained until the 26th. The board of health then removed her to the cove below the navy yard, and on the 28th sent her back to the lazaretto to undergo a more rigid *purification*. [The italics are

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ours.] There was no disease of a malignant type prevailing in the vicinity before she arrived, and no epidemic in the city. When the cargo was removed, there was an offensive smell proceeding from her hold, which afterwards became *intolerable.*

Six days after the Mandarin reached South street wharf, a suspicious case occurred. The man attacked drove a furniture car, which stood on the upper side of South street. The next day five more cases occurred, all in persons residing near or doing business in the neighborhood of South street. The seventh case was the mate of the Mandarin. Other cases continued to occur in the neighborhood of *South and Almond street wharves.* On the 23d *the captain* was seized with the disease. Then the fever gradually spread from this point.

"We look in vain (says Dr. La Roche) on all that precedes for facts or arguments calculated to show that the fever of 1853 was due to the importation of a contagious or infectious germ." Astonishing conclusion! Let our readers note carefully the history above related on pages 788 and 789 of the Appendix, and draw their own conclusions. Our own opinions are still unsettled, for the many conflicting statements of the *counsel on both sides* have confused the whole subject; but if we are ever converted to the importationists, the Mandarin case will be the most powerful agent in producing the result.

But hear Dr. La Roche's arguments against the agency of the Mandarin:

1st. "The physicians did not see anything *contagious* in the fever."

Does the man who descends a well, and is hauled out dead, give the fatal asphyxia to his kind assistant who tried to save him? Yet the poisoned air of the well certainly causes the death of all who breathe it.

2d. "There is no proof—nay, there is no reason to believe that the sailors who sickened and died on the voyage,

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admitting that they had the yellow fever, took the disease from causes existing in the vessel."

One man died on the 4th day, the other died on the 5th day of his fever. What fever is it that kills on these suspicious days? We agree with Dr. La Roche, that they did imbibe the poison whilst in port—for, as he says:

"What prevents the febrile poison from being wafted to the ship which was anchored off the town, and of course at no great distance from the latter."

And we say, what prevented the febrile poison from being wafted *from* the ship to South street wharf and Almond street wharf where she lay, "and of course at no great distance" from the poor victims, who lived just at the spot? Did the whitewashing and purification *for one entire day* prevent this result? Read Dr. La Roche, p. 753, vol. 2, who will tell you that any attempts at purification rarely succeed; and the disease will continue to prevail until the vessel reaches a cold latitude or experiences the beneficial change of a winter atmosphere.

3rd. Neither the health officer or any of his men or the watchmen on board of her, experience an hour's sickness.

This argument is worth nothing until it is proven that these persons were not protected from the disease; for if they suspected her clear bill of health, as was proven by the purificatory process of one entire day, to which she had to submit, it is probable that the fear of the fever would have prevented any one from risking their lives, unless they had already passed through the disease.

4th. "The captain and mate were not the first cases attacked, as would probably have been the case had the poison issued from the hold."

If this argument be true, the fever was not in Barry's Row in Norfolk, because Dr. Upshur, who saw the first cases, and was there constantly, did not at once have the disease, and

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indeed resisted it for *two months* after thousands had yielded to its influence. The power of resisting disease varies in different persons.

After bringing forward many facts to show that the persons attacked were surrounded by local causes which might have caused the fever to appear just at this time, the author suggested the doubt whether it was possible that the poison could have been imbibed from the vessel in so short a period as from two to five days. This short incubation, though not usual, is not at all impossible, for some seem to yield almost at once to its baneful influence, whilst others will resist it a long time. Witness the death of Dr. Hunter of New York, who arrived in Portsmouth during the late epidemic, and was a dead man in eight days. We come at last to the real *gist* of Dr. La Roche's defense of the Mandarin, and here it is.

"Three cases occurred in September in the upper part of the city, fully a mile and a half *north* of South street, but not far from the Delaware front. Here we have unmistakable yellow fever far from the infected district, and in a locality where the atmosphere, supposed to have been contaminated by a poison derived from the Mandarin, could not have penetrated."

This is indeed an argument hard to overcome. These parties had not been in the infected district; were a mile and a half from it; and the disease must have skipped over that distance to single them out especially. The Mandarin lay off *South* street, and two of three persons, a man and his wife, lived on *North* Front street, whilst the third lived in Willow street, two doors west of Front, one square *north* of the first two cases.

The importationists stand before this array of facts and answer not a word. There is but one hope. *Where did the Mandarin go when she was driven away from the place she was supposed to have infected?*

She went first to the cove below the navy yard; thence to the lazaretto to be purified. She left the lazaretto on

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the 3d of August, and went where?—"*to Noble street wharf, Northern Liberties*—where she remained until the close of the month." (Page 792.)

Where were the three cases, who had never been to South street, living?—"Two lived on North Front street, near Callowhill—the third in Willow street, two doors west of Front, and worked in Front street above *Noble.*" The Mandarin came to *Noble street wharf* early in *August*, and stayed until the month closed. These three cases were taken in *September*, and lived in close proximity to Noble street wharf—the shoemaker working in a shop only a few doors from Noble street and on Front street. If our map of Philadelphia does not grossly deceive us, we have shown that the Mandarin was near to the residence of the three isolated cases, and cannot be exonerated from the charge made against her.

We have occupied so much space in the examination of this portion of the work before us, that we will conclude by expressing our sincere admiration for the laborious and valuable labors of Dr. La Roche, and in the examination of his treatment of the great etiological questions involved, whilst we must conclude that his evident partiality for his own view has made him blind to the merits of the opposite party—yet we are satisfied that throughout the whole argument he was earnestly desirous to present both sides of the question. That he has failed to do so as fully as we would have liked, is to be attributed to that inevitable tendency to see but one side of the subject; and it may be that we have in our review fallen into a like error.

We will close, by the declaration of our high respect for the author, whom we regard as one of the chief ornaments of the Philadelphia profession, and whose contributions to the science have done credit to the medical literature of America.

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II. *An Introduction to Practical Pharmacy—designed as a Text Book for the Student, and as a Guide to the Physician and Pharmaceutist.* By EDWARD PARRISH, Graduate in Pharmacy, and Principal of the School of Practical Pharmacy. Philadelphia. 1856. Blanchard & Lea. (Received from G. M. West.)

The defective preliminary education of students of medicine has been often a subject of regret; and when we add an almost entire ignorance of the practical duties of the profession, and a want of due familiarity with the compounding of drugs, or the preparation of a prescription, which is almost universal in our country, we wonder how they succeed, as many of them do, in overcoming these fundamental defects of training, and becoming valuable members of the profession.

Nothing but the remarkable ready wit, so characteristic of our people, and the inventive talent everywhere abounding, enables our young country practitioner to work his way over all the impediments which his ignorance of the practical duties of his calling throws in his path. Many of them, with their Latin diplomas in their pockets, which they cannot read, and their learned text books, which they have not studied, cannot prepare their first order to the druggist, and stand looking around at the formidable array of labeled bottles, not knowing where to commence.

Another woeful defect, which may constantly be noticed, is the want of information amongst those who claim to be the dispensers of medicines, even in our largest towns. A boy, who has hardly learned to read his own language, is put behind the counter to prepare it may be a prescription containing the most active ingredients; and it is needless to add that mistakes of the most serious character are often times the result.

To both of the classes alluded to this book of Mr. Parrish is a godsend. The author addresses them from his dispensing office, and the whole work smells of the shop. Mr. Parrish has been engaged in the most meritorious occupa-

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tion of teaching young men how to be practical apothecaries, and this production of his pen proves that he can also teach much to every young graduate which it is all essential for him to know. A careful study of its contents will give him a familiarity with the value and mode of administering his prescriptions, which will be of as much use to his patient as to himself.

Every subject of importance, connected with practical pharmacy, is here described and explained by means of numerous and accurate illustrations, and the Appendix presents various assorted lists of drugs prepared for the convenience of the young practitioner.

One of the most appropriate presents from the worthy professor, as he delivers his affecting valedictory to his delighted graduating class, would be a copy of Parrish's Practical Pharmacy, and whilst he tells him of the noble and honorable duties which will devolve upon him, he also teaches him how to make the pill which is destined to be the foundation of his future practice.

III. *Principles of Human Physiology, &c. &c. &c.* By WILLIAM B. CARPENTER, M. D. F. R. S. F. G. S. A New American, from the last London edition—edited, with additions, by FRANCIS GURNEY SMITH, M. D. Philadelphia. Blanchard & Lea. 1855. (Received from A. Morris.)

Professor Carpenter, at least, has no right to complain of American publishers or the American medical public. One edition after another of his works has issued from the well known press of Messrs. Blanchard & Lea, who have received the well merited thanks of the author "for their honorable liberality, which they have on all occasions evinced in their arrangements with him," whilst the ready sales accorded to him testify to the high appreciation with which his various physiological works are regarded by the American profession.

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The author having determined to divide his labors under the three heads of general, comparative and human physiology, is enabled to reduce the size of the present edition, by throwing out much of the matter more properly appertaining to general physiology. He thus passes over the summary of animal chemistry in the edition of 1853, as well as his examination of the structure and action of the animal tissues; and by confining his attention to human physiology proper, he has rendered his present work probably better adapted to the wants of the student with whom he has always been deservedly popular, whilst we still prefer his edition of 1853 for the purposes of reference, as containing his sketch of the connections between chemistry and physiology, which we regard as of great value.

We are, however, promised new editions of the two other divisions of physiological science adopted by our author, which will present, under three distinct heads, all of his contributions in this department.

The book is handsomely illustrated with nearly three hundred engravings, and has many additions of value, selected from the labors of other physiologists. We doubt not it is destined to retain a strong hold on public favor, and remain the favorite text-book in our colleges.

EDITORIAL AND MISCELLANEOUS.

Salutatory.

The old year, with its joys and sorrows, has faded slowly away, and the wholesome customs of our ancestors teach us to proclaim a truce for a brief period with all care and labor, and in the enjoyments of the Christmas festival, to gather a new fund of energy and hope to prepare us for the duties of the future. Friends long separated, relatives who have wandered far away from the old homestead, now clus-

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ter around the family table, and even the stranger who is passing by is greeted with all the affectionate warmth of an Old Virginia welcome.

We gladly seek this propitious moment to make our salutations to the readers of this journal, and hope to meet with a cordial reception. There are many whom we now address for the first time; and whilst we are cheered by the presence of those who have always kindly encouraged our efforts, we yet hope, under the genial influences of the season, to be regarded by all as an old and valued friend.

Yet, why should we hesitate to execute the task before us? We plead in behalf of the noble science which claims our common fealty, and ask you to add the results of your observation and experience to the common fund. When the profession everywhere is earnestly seeking the acquisition of medical knowledge, shall we idly sit and enjoy the fruits of their labors? When Virginians lead our armies to victory, and guide the destinies of the nation, can they not also enter upon the boundless realms of science, and contribute their proportion to swell the ever advancing tide of knowledge? We confidently appeal to the profession of our own state, and to the medical men of the south, who have heretofore, with but few exceptions, yielded to a melancholy apathy, and ask them to unite with us in the praiseworthy effort to rear up in our midst a southern medical literature.

These words will be read by more than *one thousand* physicians actively engaged in the performance of their professional duties—earnest, intelligent men, who study disease patiently, think originally, and when the occasion demands the sacrifice, have not hesitated to devote their lives to the fulfillment of their sacred duties. Let each one determine to place on record a single observation of the many which the year's labor will afford him, or even report one case suggestive of original or useful ideas, and this journal will have accumulated, before the termination of its next volume, an immense mass of valuable information in all the departments of medicine. And now, when we see the lead-

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ing men in our ranks passing rapidly away, and in the grave of each lies buried the stores of knowledge and experience which a life time had collected, we bitterly mourn this fatal neglect of a great duty, and the more earnestly strive to rouse the many who still remain to avoid the same error.

We offer our united journal to the profession of the south, who feel the importance of entering upon this much neglected field, and ask them to make it worthy of its name, and even to regard it as their own impartial representative. We have no personal interests to defend, or private animosities to gratify; we throw all our hopes of future prosperity on the great mass of the profession, and with them alone rests the question of success or failure.

We have opened our hearts to you all—old friends and new; hear us for our cause. Without your approval and co-operation, we may become a respectable digest of the literature of the day, but we can never be a *Virginia book;* but give us your active aid and sympathy, and our journal will assume an honorable position on the records of the science, and prove itself a not unworthy representative of the southern medical public.

Died, in Petersburg, December 6th, 1855, *in the* 40*th year of his age,* JOHN FREDERICK PEEBLES, M. D.

DEATH has been indeed busy amongst us in the last few years, and the pages of this journal have too often recorded the loss of some worthy and valued friend. The aged man has gone to his tomb in the ripeness of his years; the young and ardent spirit has fallen like a hero in the midst of the pestilence. The mournful fate of *Johnson* is still before us, and the memory of *Selden, Upshur* and *Gooch,* tells the sad tale of "dear friends untimely killed." But now the blow has fallen nearer home—our intimate associate and faithful friend has gone to his long account; our zealous adviser and colaborer in all the difficulties of the editorial life has left us

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in the midst of the struggle, and the earnest and enthusiastic lover of science sleeps quietly in his early grave.

There is no member of the profession in this state, who has won for himself a more enviable reputation than the subject of this notice. A native of Dinwiddie county, Virginia, Dr. PEEBLES entered the profession about sixteen years ago, and offered his services as a practitioner of medicine to the inhabitants of Petersburg. His devotion to his calling and accurate knowledge of the science soon gained the respect of the entire community, whilst his genuine honesty and simplicity of character won the affections of all who knew him.

In spite of a rapidly increasing practice, Dr. PEEBLES has always been ready to contribute the results of his experience to the medical literature of the day. His various communications to the American Journal of Medical Sciences, the Stethoscope, the Virginia Medical and Surgical Journal, are marked by great correctness of judgment and a rare felicity of expression. His admirable paper on Displacements of the Uterus, to which was awarded the Fiske Fund Prize for the year 1853, is too well known to require any commendation at our hands, and he followed up this success, by carrying off the prize offered by the Medical Society of Virginia in the following year, which essay having been published in the Stethoscope, is familiar to our readers.

In April 1854, Dr. PEEBLES became an associate editor of the Virginia Medical and Surgical Journal, and whilst his failing health and large and fatiguing practice prevented him from contributing regularly to the pages of that periodical, yet many of its best reviews are from his pen, all characterized by the peculiarly lucid style and accurate knowledge he has ever displayed in his literary efforts. His devotion to his profession has brought him to his premature grave, and he leaves behind him a whole community to deplore his loss.

The following resolutions of the Petersburg Medical Faculty, who had known him longer, although they could not have loved him more, express, in language better than any

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we could select, the high appreciation with which he was regarded by his daily associates:

Tribute of Respect.

At a meeting of the Petersburg Medical Faculty, held on the 6th inst. at the office of Dr. P. C. Spencer, president, the following preamble and resolutions were adopted:

Whereas it has pleased an allwise Providence to visit us and this community with a most afflicting dispensation, in the death of Dr. John Frederick Peebles, a worthy member and friend, who, on the morning of the 6th instant, was gathered to his fathers; in memory of his many virtues, and his exalted character as a man and a physician, the Petersburg Medical Faculty do unanimously adopt the following resolutions:

Resolved, that we deeply deplore the loss of one whom we regarded as an ornament to our profession, and whose social virtues had endeared him alike to us and a large circle of friends.

Resolved, that we earnestly sympathize with his relatives and friends in this sad bereavement.

Resolved, that in respect to his memory, we will attend his funeral in a body and wear the usual badge of mourning for the space of thirty days.

Ordered, that a copy of these resolutions be furnished the family of the deceased, and be published in the papers of the city.

P. C. Spencer, *Pres.*
C. F. Couch, *Sec.*

Excerpts.

Aphorism of Galen.—"Immo vero ex physiologia omnes antedicti sapientiæ professores ostendunt, neminem posse morbos commode curare qui corporis universi naturam non perspexerit."

The Physician's Bride.—Not unfrequently medical men repent of their choice of a profession. This arises from their being unable to find a compensation in the science for the daily toil in the practice of their art. A physician, to be happy, must love science with the ardor of a lover. She must fill his whole soul; she must be incorporated with his being; and then she will daily appear to him new and interesting. Medicine is exciting precisely because no man can ever arrive at a complete know-

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ledge of her; and in this she differs from sciences which are complete in themselves. Whilst in them, at best, one yawns over quiet, undisturbed possession, in medicine there is daily new life, new excitement. I shudder when I fancy myself a jurist or a theologian. Heinrich Heine says somewhere, the Frenchman loves liberty as his bride, the Englishman as his wife, the German as his grandmother. I say, that jurists, mathematicians, theologians, etc. can at most love their science as a wife; to the physician alone it is granted to love his as a bride—with all the enthusiasm of first love.—*Pauli.*

Fever.—It is the *gastro-enterite*, cry the disciples of Broussais, and there is no cure but the leech. It is cerebral inflammation, respond the followers of Clutterbuck, and the only remedy is venesection. Purgatives are the true treatment, proclaims the Edinburgh school. They are irritating and dangerous, replies that of London. In fact, all are equally right, and all are equally wrong, if they fail to note times and seasons, the nature of the epidemic, and the characteristic tendencies of the patient's constitution, his powers to bear the operation of medicine, and his ability to resist the tendency to death. There can be no treatment of fever by physic but in studying the *juvantia* and *lædentia* of the case—cultivating the first, eschewing the last, and never forgetting that there is a mighty power always operating in your favor—the *vis medicatrix naturæ.* Do not thwart her beyond the mark, and she will get you through difficulties with which, without her aid, you could not cope. But the physician, who believes that he possesses beyond these, medicines of specific power in fever, (such as quinine, turpentine, and nitrate of silver,) really should have his license suspended, and himself put under cure until the monomania subsides.—*Inspector General Fergusson's Notes and Recollections.*

Mortality Statistics of the United States.

We are indebted to the politeness of J. S. B. De Bow, Esq. the superintendent of census, for the proof sheet of the mortality tables, which have been prepared with great labor and care from the statistics of the census of 1850. There is an immense mass of useful information to be derived from an examination of these tables, and a comparison with the statistics of other countries. At some future time we intend to institute such a comparison, and only design at present to notice some of the most striking facts to be observed in these interesting documents.

The proportions of deaths to population in the United States give us the cheering information that our chances of living are better than those of any other civilized people on the globe. Our population being 23

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millions, whilst the deaths occurring in one year amount to 320,000, the ratio of deaths to population is about 1½ per cent. The same calculation in England shows the ratio to be above two per cent. and in France at nearly three per cent. It would thus appear that in spite of the gloomy forebodings of those who warn us against our bad habits, neglect of sanitary precautions and violations of all the rules of hygiene, we still seem to outlive sturdy John Bull or the soup maigre Frenchman. This is made more forcibly to appear when we deduct the deaths from cholera occurring in the year of the census, (1849,) amounting to 31,000, which would still more reduce the ratio.

The proportions of deaths in the northern and southern states, show a remarkable difference in favor of our climate and region of country, and we are happy to inform our readers in Virginia and North Carolina, that they are destined to outlive their abolition brethren of New York and Ohio, to a very comfortable extent. The population of Virginia and North Carolina is 2,270,000, and the number of deaths in the census year 29,000, the ratio being thus 1.03 per cent. Of the five millions of people inhabiting the large states of New York and Ohio, nearly 75,000 annually die, the ratio being 1¼ per cent. Yet strange to say, although we thus perceive that our own climate is so favorable to longevity, we have a larger proportion of medical men than almost any portion of the Union.

The number of persons who reach the age of 100 *and upwards*, will satisfy us more completely as to the correctness of the above statement. In Virginia and North Carolina, we observe 638 persons who were 100 and upwards. New York and Ohio, with more than double the population, can show but 146! Again, in the two southern states, 129 persons died, during the census year, over 100—in the northern, only 41.

A nut for the abolitionists.—Whilst the white population outlive all the rest of mankind, the blacks of the south far exceed them in the duration of their lives, and in spite of horrible barbarities, scourgings and persecutions to which they are subjected, our tables show how wonderfully long they stay here to bear it. There are in the Union 19,500,000 whites, and 3,600,000 blacks—more than five to one. We find 787 whites over 100 to 1,768 blacks—thus making the proportion in favor of the unfortunate African more than ten to one! We are now able to account for the heretofore extraordinary fact that almost every old negro in Virginia was at the siege of Yorktown, and remembers well "Massa Cornwallis and the great Gineral." However this may be, it is evident that they live longer than any people on earth—our philanthropic brethren to the contrary notwithstanding.

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VARIETIES.

Medical News and Items.

STATISTICS.—The *Gazette Médicale* for Nov. 3d, gives an extract from a curious document published by the French minister of commerce and agriculture, from which it appears that there now exist in France 37,662 blind persons, or 105 in 100,000; 73,063 persons blind in one eye; 29,512 deaf-mutes; 44,970 lunatics; 44,382 persons affected with goitre; 44,619 hunchbacks: 9,077 persons who have lost one or both arms; 11,301 persons who have lost one or both legs; 12,547 persons affected with club-foot.

GERMAN MEDICAL SCHOOLS.—It is customary in the larger universities of Germany to take possession of professors who gain celebrity at the smaller schools, by offering greater inducements than the latter less opulent institutions can afford. The smaller schools suffer in one point of view, from this kidnapping, but on the other hand, incentives to exertion are constantly held before their teachers. Giéssen has successively lost Liebig and Bischoff, and is soon to be deprived of the distinguished Vogel, who has been called to Halle, in Prussia. Ludwig of Zurich, in like manner, removes to Vienna, as professor of physiology at the Academy of the Emperor Joseph.—*Gazette Médicale.*

PROFESSORIAL APPOINTMENTS.—Dr. Laycock has been appointed to the chair of clinical medicine in the University of Edinburgh. Dr. Natalis Guillot is appointed one of the two professors of medical pathology of the Faculty of Paris, to fill the vacancy occasioned by the death of Réquin.

NECROLOGY.

QUEVENNE.—The *Archives Générales* for November 1855 announces the death of one of the most distinguished representatives of pharmaceutical science, M. Quevenne, chief pharmacien to the hospital *La Charité*, at Paris. The researches of Quevenne on milk, iron and digitaline are too well known to require any eulogy from us. Besides his original monographs on these subjects, this excellent chemist assisted in numerous investigations in anatomy and pathological physiology. He was a man of the highest character, and of remarkable amiability. He was always ready to assist with advice or experiments the physicians who sought his laboratory. All who knew him appreciated his modesty, his zeal and his ardor in the pursuit of knowledge.

BERRUTI—SOMME.—Prof. Berruti and Dr. Sommé, two of the oldest of the Belgian surgeons, died recently, the former at Ast, the latter at Antwerp..

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METHODICAL ORDER,

ıe of Paris.

ED BY O. A. CRENSHAW, M. D. RICHMOND, VIRGINIA.

OTHER CAUSES THAN PREGNANCY, IS COMMON.)—(P. Dubois.)
function, (rare,) perverted taste, (common.) CONSTIPATION,

pearance, projection, papillary tubercles, colostrum, milk.—
salts. SKIN, (moth, coloration of the linea alba.) SALIVARY

e.)
BLOOD. (Diminution of the red globules and augmentation of

upwards, *gradual, to an extent equaling the softness of the vagina.*
ternal orifice closed *until the period of labor.* Exceptionally (not
e third of the last phalanx to enter. (Pajot.)
, or rather like a candle extinguisher. (Pajot.) External *orifice*
osed except in rare cases. At six months the finger will pene-

ished. (Stoltz.)
nent and higher at the end of pregnancy.
result of the inverse inclination of the body.
tchouc.)

ed by the finger of the accoucheur, placed either in the anterior
lf. (Velpeau, Depaul.)

w the EPIGASTRIUM.
. . . .
. . . .
e the UMBILICUS.
. . . .
. . . .
f the PUBIS.

uctuation *sometimes very distinct.*
OVOID.
ceptionally from left to right.)
lateral wall a little anterior.

the lateral parietes, (the most common.) SUDDEN JERKING
ced on the abdomen,) certain, BUT MUST BE FELT BY THE

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CONNECTION WITH CHILD-BIRTH.

Cazeau, &c.

INDICATIONS AND TREATMENT.

PELVIS
'RACTED.
eaving
NCHES,)
east in
est diam-
eter.

VERTEX. — WAIT FIVE OR SIX HOURS AFTER COMPLETE DILATATION. The head being at the superior strait. At the inferior strait, wait two or three hours. APPLY THE FORCEPS. *I, myself, rather direct to wait as long as the contractions continue, and the condition of the mother and child is not dangerous.*

FACE. — Endeavor to convert into presentation of the vertex, FREQUENTLY IMPOSSIBLE. Apply the forceps without waiting as long so for the vertex.

PELVIS. — WAIT. Then moderate and guarded traction of the extremities. Artificial delivery of the head with the hands or the forceps.

TRUNK. — Attempt version by the head, then act as for the vertex presentation, if cannot succeed—version by the feet, then as for presentation of pelvis—pelvic version in all these cases according to Mad. Lachapelle and M. Simpson.

INCHES)
nost and
NCHES)
least.

IF THE CHILD IS ALIVE, wait some hours after the dilatation, then use the FORCEPS; if do not succeed, wait two or three hours longer, then apply forceps again—sometimes make a third application of them—finally, PERFORATE THE CRANIUM, wait, or better, apply the CEPHALOTRIBE. IF THE CHILD IS DEAD, use the last means sooner. Premature artificial accouchement at 7½ or 8 months—particularly if in former occasion the woman has not been delivered spontaneously or by the forceps. *P. Dubois exacts this last condition.*

NCHES)
nost and
INCHES)
least.

PERFORATION OF CRANIUM and CEPHALOTRIBE, or symphisiotomy, (not practiced now,) after having waited as long as we can hope any thing from the contractions without compromising the condition of mother. (*Artificial delivery at 7 or 8 months.* Low diet to the mother.) (Moreau, Depaul.)

If in these two cases of contracted pelvis, the fœtus presents any other part except the vertex, attempt to bring this last to SUPERIOR STRAIT and act afterwards as directed.

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VIRGINIA MEDICAL JOURNAL.

THE STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL

COMBINED.

FEBRUARY 1856.

ART. I.—*Obstetric Tables.* By Dr. PAJOT, Agrégé Professor to the Faculty of Medicine, Paris. Translated from the French and Arranged. By O. A. CRENSHAW, M. D. and J. B. McCAW, M. D. Richmond, Va.

THERE is no one of the junior members of the Parisian faculty who has, at an early age, achieved a more honorable reputation than the author of the Obstetric Tables, which we propose to translate for the first time into the English language. As assistant professor of the obstetric art in the Ecole de Médecine, Dr. Pajot has, at the age of thirty-five, obtained a most prominent position in the public estimation; and his thronged lecture rooms and large class of private pupils attest his popularity with the students—whilst his bold and original views are destined to make his name well known in the annals of midwifery.

In presenting the five tables which we have translated and arranged with great care, we design to append a few explanatory remarks, although the admirable simplicity and methodical arrangement, pursued by the author, hardly render such an explanation necessary. The complete set offers to

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the student of medicine or to the practitioner a perfect synopsis of all the difficulties of midwifery. He has before him at a glance the obstacles which he may have to meet, and the method by which he can overcome them. He may in a moment ascertain the value of a doubtful symptom, or explain the cause of an unexpected phenomenon. In addition to this, these tables give us the condensed literature of the subject down to the present day, more especially as regards the continental school of medicine; and wherever there is a difference of opinion held by the great lights in this department, each name is especially noted. We have thus at command an epitome of obstetrics, prepared after the best authorities, and also the various doctrines and opinions on all special points, held by such men as Depaul, Du Bois, Stoltz, Velpeau, &c. whilst the vast practical experience of the celebrated *sages femmes*, Mesdames Boivin, La Chapelle, &c. is appealed to, in order to make the work perfect in all its parts.

TABLE I is devoted to the SIGNS OF PREGNANCY, derived from the *general symptoms, the touch, palpation, auscultation and percussion.* Under the head of *general symptoms*, we can study the functional derangements caused by pregnancy, giving to each its relative value. The acknowledged dexterity of the French school in the art of touching, and the practice of the ballottement, give additional value to the remarks of Dr. Pajot. The various sounds revealed by auscultation, with the explanations of the causes of each, and the use of percussion, are included.

TABLE II is a classification of the PELVIC DEFORMITIES, including the deviations of the pelvis from the natural standard. The contracted pelvis is divided into the one perfect in shape and only diminished in size, whilst the other, although sufficiently large, is disproportioned in figure and distorted in form. The causes of pelvic deformities, the measurement, the diagnosis and prognosis, are noticed and explained, and the indications and treatment admirably condensed. The treatment of both mother and child is

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alike detailed, and the practice of the eminent obstetricians of the continent is quoted whenever necessary.

TABLE III.—Here we have a synoptical table of the treatment of HEMORRHAGE, based upon the views of Du Bois, Chailly and Pajot. The subject is discussed very clearly, being divided into hemorrhages occurring before and after labor, with the uterus dilatable or nondilatable, and when the membranes are ruptured or entire. The table closes with the treatment of hemorrhage from attachment of the placenta over the os uteri. The whole is a good resumé of the French practice; and although we do not agree with the author in his fondness for the tampon, we yet regard his table as containing matter of great interest and value.

TABLE IV discusses the important subject of TURNING, and commences with some admirable maxims and general directions, which we cannot too highly recommend to the reader's careful study. Pajot divides version into three stages: the introduction of the hand and examination; the evolution of the child, and the extraction and delivery. Under the first head he details all the difficulties which may be met with in the examination; the complications and malpositions of the fœtus, with directions for forming a correct diagnosis. The manipulations necessary in the second and third stages are described with great clearness and brevity.

TABLE V.—We do not know of a more masterly condensation of an immense amount of information in a small compass than we have in the fifth table of Dr. Pajot. It is devoted to a description of the APPLICATION OF THE FORCEPS, under all the circumstances which could possibly occur, and to the inexperienced practitioner especially, will be of great value. The method of applying the instrument, the proper hand to use, and the position to be assumed; the method of applying force and of extraction; and the cautions to be observed are accurately related. The oblique as well as the direct application of the forceps is treated of, and the use of the instrument in each presentation. All

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the complications and difficulties are guarded against, and the table closes with the rules to be observed in the operation of *cephalotripsy and craniotomy.*

In preparing these elaborate tables for the use of the English reader, we believe that the opportunity thus afforded them of seeing grouped together the important and difficult points in the practice of obstetrics, will render our labor appreciated by the profession of our country; and we hope that they will be found instructive and useful to the student as well as the practitioner of medicine.*

ART. II.—*Cases of Diphtherite—Croupe Chèz l'Adulte.* Reported. By CHARLES MINOR, M. D. Charlottesville, Virginia.

1843, May 11.—Was called to see, at *****'s plantation, located on northwest side of Southwest mountains in Albemarle county, Virginia, several cases, which were pronounced by the master and a physician to be "putrid sore throat," or a form of scarlet fever.

CASE I.—Negro girl *Betsy*, age 14, taken 7th May, and treated on 9th with red pepper gargles, and blisters to outside of throat. Blistered surface very sore; has gangrenous aspect. Fauces nearly closed by deposit of yellowish looking matter, (much like buckskin,) which extended down the pharynx as far as the eye could reach. It assumed the form of the parts it invested. The voice was interfered with, as is usual when the fauces are obstructed, and the swallowing difficult, so that only fluids or pultaceous material could be passed, and these were sometimes rejected through the nose. Breath very offensive; teeth covered with sordes; tongue with heavy brown fur, rather dry, almost black; respiration

* We hoped to have presented our readers with the complete set of tables in this number, but the delay incidental to their translation and arrangement has compelled us to postpone Nos. III, IV and V until the March issue.

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hurried, but no cough nor croupal breathing; pulse feeble, quick in the beat, but not frequent, only about 70; skin rather cool, and disposed to sweat freely; muscular strength greatly reduced; heart and nervous system so impressible that when this girl rose to show me her throat, she fainted; no diarrhœa, nor other sign of other local lesion, except of throat; appetite indifferent. Although no diarrhœa existed, there was a certain mobility of the bowels, readily and excessively acted upon by a small dose of aperient medicine.

Case ii.—*Martha*, age 20, mother of one child, now at breast. Visited at same time, in same room of a double cabin. Throat not very sore; swallows quite well; tonsils large; slightly covered with the yellow matter. Sick since the 9th May. Some debility; pulse 90; skin dry; bowels tardy, but yet easily moved, they say.

Case iii.—*Maria*, age 12. Buffy looking deposit extends down pharynx, far as eye can reach; respiration hurried; swallowing and articulation much incommoded; no croupal breathing; no cough, nor other sign of local lesion; tongue covered with brown fur; teeth black with sordes; pulse feeble, about 75.

Cases iv, v, vi, vii, viii, ix, x.—Some existing now, and others occurring afterwards, in a few consecutive days, were only copies, more or less intense, of these.

The treatment applied in them all was nearly the same. Fauces mopped with solution of nitrate silver, 5 grs. to the ounce of water, repeated two or three times a day; poultices kept constantly warm and moist to the throat; gargles of a solution of alum sweetened with honey, used whenever the throat was dry, and the patient desired it; bowels gently moved by castor oil every other day, and promptly restrained by laudanum, if action threatened to be excessive. Diet, mush and milk, mutton broth, with rice, &c.

May 24.—All the cases were either well or rapidly convalescent. The first, Betsy, coughed up, now and then, very large quantities of sanious matter mixed with the false membrane, as soon as the caustic had time to act—that is to say, after a few applications; and generally, it was apparent that

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this agent enabled the mucous membrane to throw off the deposit with promptness.

I will remark, in passing, that my experience does not confirm the opinion of some users of lunar caustic, that less than 40 grs. to ℥i is a nullity, applied to mucous membranes. I am rationally persuaded that a five or ten gr. solution has been seen to do all in these cases that 20, 30 or 40 grs. can do, for good.

The pulse, in all the cases, was marked by feebleness; ranged between 60 and 75, but did not require other stimulants than nutritious food and external warmth. These were often required at irregular times, the feet and hands growing cold, and demanding hot applications. Had these simple agents been omitted, it is probable that occasional means of greater power would have become necessary. This my experience teaches.

The double cabin was occupied by 12 persons, including a woman of 75, and the infant. All had the disease except these and one young adult.

It attacked only the inmates of this house, and two other individuals who lived 400 yards off, but who had "used" at this cabin. Other houses occupied by negroes on the same hill side, distant from the infected one not more than 50 or 75 yards, were exempt.

Case xi.—One of the two, not tenants of this cabin, was not my patient. The master afterwards informed me that the treatment which I adopted was not applied. Gargles of red pepper and bleeding from the arm were used, and the poultices with lunar caustic omitted. He was 16 years of age, and died with croup symptoms.

Case xii.—June 28, 1843.—Visited for consultation with Dr. **** a son of Mr. **** (residing on the eastern slope of the Southwest mountains, about four miles from the locality of the other cases,) age 14. Taken 25th inst.

In evidence of the unusual character of the disease, the attending physician, in a very extensive practice of near 30 years, declared he had never seen it before, although he had then recently read of it in the journals, and identified it.

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Deposit on tonsils and fauces very heavy and unbroken, extending down the pharynx as far as eye could reach; great difficulty in swallowing and enunciating, but puts out tongue and opens mouth very well. This latter fact may indicate generally the extent to which the tonsils or other glands, in vicinity of the maxillary articulation, are swollen. *Skin quite hot; pulse full,* and 96—100; bowels not adequately moved for two days past. No croupal breathing; a little cough. I underscore the *hot skin,* and *full pulse,* because it is an exception to the rule in my experience.

My friend had given 6 grs. calomel three times, at intervals of two hours, producing two stools quite copious, and nearly pitchy black; caustic silver, in solution, to fauces, and a blister to throat. This surface very sore, but not gangrenous, nor covered with the diphtheritic deposit, as is likely to happen.

Calomel 5 grs. given again at 9 P. M. In morning had another stool of same character; pulse softer and less frequent.

Poultices and caustic were continued, but I did not see the patient again. He recovered with remarkable aphonia, which lasted a long time.

A negro girl had the disease afterwards, and died with croup. Post mortem.—Dr. **** attending physician, found large masses of false membrane in the larynx and trachea.

Some other cases occurred in this family, one of which I saw on the visit above mentioned. This child, about eight or nine, was on his feet, not known to be sick, until, on my suggesting the insidiousness of the affection, the fauces were examined, in this one of the children at random, and discovered to have the characteristic deposit to an alarming extent. Bretonneau, Ryland and Louis teach us this in relation to the "croup of the adult," and warn us of its liability to reach the larynx before one is aware of its existence.

There was no eruption visible in either of these cases, which, being white, furnished opportunity to discern it.

Cases xiii, xiv, xv.—During 1843, I treated many other cases. A lady, 50 years old, (boarding under the roof of a

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family, embracing four or five young persons,) was seized with this affection of the fauces, and her two daughters also had it. The whole disease manifest, consisted of extreme debility, foul tongue, and the false membrane on the fauces and pharynx. The other family under the same roof, escaped. The local treatment with lunar caustic, 5 or 6 grs. to ℥ i water, poultices and very gentle aperients, constituted the whole management of them all, and it was required for from five to ten days. It was conspicuous how promptly the caustic and poultices relieved the disagreeable and painful sensations in the throat. No eruption visible on the skin.

Case xvi.—1843, September.—A negro boy six years old, presenting the usual symptoms; circulation feeble; fauces covered with the false membrane, but no croupal cough at first. The caustic, &c. recommended were not used by reason of the boy's resistance, and the next day I found him suffocating with croup. He died, stoutly standing out to the last against the application of every remedy. An example, I may observe, of rebelliousness in children which we too often see prove fatal.

Case xvii.—A girl eight or nine years old, one of two or three who were attacked on another plantation, about same date, presented the same symptoms at first, and upon the accession of croupal cough and breathing, was promptly treated with small doses of calomel in addition to the caustic, &c. two grains every two hours, and with emetics when the phlegm demanded it. The child recovered.

1844, October 14.—Called to see family of * * living on a high, poor ridge, in comfortable circumstances, eight miles from other neighbors.

Case xix.—A son, 19 years old, had returned on October 4th from Richmond. Was taken sick with symptoms such as marked the cases I had now before me. On the 6th a physician prescribed some medicine, regarding it lightly, (the family said)—red pepper gargles, &c. The patient died October 8th with croup.

Case xx.—October 8.—Another brother, 17 years old, who had also been to Richmond, and returned with the other,

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was seized. A different physician sent for. Prescribed blister to throat, red pepper gargles, emetics and lunar caustic. Died October 12th of croup. This medical gentleman, being in attendance at my visit, assured me that the cases we had yet on hand, five in number, all white, were affected precisely as the two first, and that some of them were as bad as the one he had treated, who died.

Cases xxi, xxii, xxiii, xxiv, xxv.—All of the five had had emetics and red pepper gargles, and one had been bled. Sick now two days. The red pepper tea was discontinued, and instead, lunar caustic solution, 5 grs. to ℥ i water, poultices, gargle of alum, sage tea and sugar; bowels to be gently moved every other day by castor oil, never by Epsom salts, which I supposed to be noxious.

17th.—All were doing well, the diphtheritic membrane on fauces falling off; voices less obstructed; deglutition better, &c.

Cases xxvi, xxvii.—There were two new cases, one the mother, 50 years old. The same treatment was continued.

18th.—All doing well again, and all recovered.

Other cases occurred about this date in the same neighborhood, which I treated upon the same general plan, and all recovered. No eruption on skin in any case.

Remarks.—Since 1844, I have never seen a case of diphtherite. It is said to have prevailed in subsequent years in the northwest of Albemarle county.

These have seemed to me, incontestably, true diphtherite of Bretonneau, croup chez l' adulte of Louis, &c.

Ryland states, p. 120, Dis. of Larynx, that he has known diphtherite to occur in conjunction with scarlet fever and measles. It is true that scarlet fever and typhoid fever were rife during both of the years 1843 and 1844, in the same neighborhood. But all the examples I have cited wanted the characters of scarlet fever and measles, there being no eruption over the body, and no desquamation. The tongue and palate did not present the characteristic redness of scarlet fever—these parts exhibiting the eruption, I have thought,

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when there was none on the skin, and even when the depression was so great in the beginning, that reaction did not take place at all.

Neither did the parts seem to suffer loss of substance when the coat of buff was removed, as in scarlet fever. There was no ulceration, the sanious matter cast up being only extravasated blood and effused lymph. Ryland Dis. Lar. p. 123.

The occurrence of croup as the result or termination of the incipient symptoms which have been described, must be, when superadded to the other evidences, entirely conclusive: especially when so many cases arose in the progress of two years, having the same early symptoms, and the same resulting croup.

It is quite probable, however, that there is a relation between scarlet fever, typhoid fever and diphtherite. What elements may compose that relation, whether it be a similarly constituted miasm, or miasms different in constitution but analogous in affinities, I shall not undertake even to surmise.

The tendency of diphtherite to croup—the formation of a genuine croup membrane in the larynx and trachea—a membrane whose essential element is an exceedingly plastic lymph—would seem to distinguish it positively from those diseases. Such a predominance of fibrine would set it far apart from scarlet fever and typhoid, which, according to Andral and Gavanet, having a minus quantity of fibrine, notwithstanding their leading anatomical characters might be presumed to place them among the phlegmasiæ.

In sort of collateral evidence of this difference, it is to be observed, moreover, that when the croup symptoms set in, the antiplastic power of mercury is invoked, and is recognized by the highest authorities as being alone serviceable. Ryland says that when the larynx is invaded, and croup has supervened, "the only constitutional remedy not quite destitute of efficacy, is mercury." And the only instance which Louis records as cured, was the only one in which mercury

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was tried. In this, croup symptoms (such as alteration of voice, pain in larynx and trachea, fits of suffocation) occurred and were relieved.

In view of this proclivity to croup, a question naturally arises as to the anticipative use of mercury. Should we administer calomel while the constitutional symptoms appear to be only those of depression; cool skin, often relaxed to perspiration, feeble pulse and muscular weakness, blisters apt to be gangrenous, purging and blood letting, hurtful? The question has been answered variously. For myself, I have been satisfied by my experience, as already detailed, that in a very large majority of cases, early submitted to treatment, the local appliances, lunar caustic, gargles and poultices, will avert bad results, and that mercury is therefore not needed until the emergency arises. When the larynx seems to be assailed, then it will be time enough to introduce that agent. This is stated as a general rule, to which of course there may be exceptions.

Red pepper gargles and blisters have appeared to be actually injurious. Alum seemed to exert somewhat the same effect as lunar caustic. If I may be permitted to speculate upon the reason of what seems to be a fact, I would say that perhaps nitrate of silver exerts upon the capillaries of the mucous membranes of the fauces the same tonic and sedative effect which we see it produce upon the conjunctiva in a state of overfullness. It may enable the vessels to disgorge themselves, and so arrest the effusion of plastic lymph. Nay, more—in virtue of its power over the nerves, as exemplified repeatedly in epilepsy, the nervous extremities of the very part may be so wrought upon as to modify their susceptibilities and functions. In croup, an emetic of alum exerts an effect upon the tough membranes, loosening and dislodging them, which is hardly explicable by the mere succussion of the vomit. There may be an action set up, by a sort of continuous sympathy, along the pharyngeal and laryngeal tracts, which unloads the turgid capillaries, and alters the function of the submucous structures that give rise to the adventitious membranes.

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Art. III.—*On Surgical Operations.* By Senex, of Virginia.

The department of surgery is universally admitted, by the enlightened members of the medical profession, to be the most difficult branch of the whole round into which medicine has been distributed as a practical profession; whether contemplated in reference to the numerous and diversified diseases that fall properly within the province of the surgeon, or the many painful and dangerous operations demanded at his hands for the cure of those diseases. Hence, in all probability, is furnished a satisfactory reason why so few medical men, of the large number that study medicine as a profession, become operative surgeons. The duties of a practical surgeon are indeed onerous and important, and well calculated to deter beginners, in the onset of their professional career, from attempts to operate in dangerous cases, and even from witnessing painful and bloody operations in some instances. With beginners thus constituted, nothing but a sense of duty or humanity could operate as inducements to attempt a critical and hazardous surgical operation; and in many instances humanity has had to lament the insufficiency of these powerful incentives, where life might have been saved by a timely surgical operation. So revolting and horrifying to the feelings are many of the operations, as to have caused veteran surgeons in some instances to pause and to waver before consenting to undertake them, or even to advise such operations.

It is highly probable that every humane and conscientious surgeon is the subject of such emotions, in greater or less degree, before commencing or even advising painful and dangerous operations; and, if it is a weakness to yield to such a struggle of feeling, the victory is due to the surgeon's humanity chiefly; and while it does not compromise his moral courage, is creditable to his heart as a man, and generally leads to the safety of the afflicted, as it does to virtue and benevolence. A sensitive and humane surgeon, who

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can feel sincerely for the sufferers, not only in the contemplation of their cases, but when under his hands during an operation, is sure to inflict little needless pain with them; and as certainly will perform his duty faithfully, because he acts conscientiously. He will never advise or undertake a needless operation; nor will he countenance or excuse such operations in the hands of other surgeons. Every surgical operation is to be regarded, in a degree, as an example of human immolation, offered at the altar of health and life; and it should be approached, accordingly, with feelings of honest sincerity on the part of the surgeon. That is, he must believe conscientiously that no other measure will arrest the disease; and that by timely resort to it, health and life, in all probability, may be preserved. These are the cardinal points of operative surgery, from which no surgeon can vary, without compromising his character for honesty, and degrading his noble profession—a humane and benevolent institution, established for the cure of human diseases—to the level of a contemptible stockjobbing occupation, whose objects are, speculating in human blood and human suffering for pecuniary gain or eclat.

A surgeon is not even justifiable in the undertaking of an operation to gratify patients or their friends, when he believes no benefit to the sufferer can possibly result from it. Such an operation would be cruel in the extreme, because its object would be the gratification of hopes founded in the grossest ignorance of the case for which it might be performed, at the expense of great needless bodily suffering to a fellow being.

The most cruel and heartless operations, however, that a surgeon executes, are those designed chiefly for eclat or self-laudation; and there is too much reason to believe that many, nay very many of the operations of the present times are of this description. It is by no means uncommon "now a days" to hear of amputations of the limbs, especially the lower, for ulcers, not involving the bones, nor possessing very questionable or menacing characters.

Some few years since, the writer, during an excursion into

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a neighboring county, chanced to meet with a servant who had lost a leg; and upon interrogating him as to the cause, he replied that it was cut off on account of an ulcer on the shin, not larger than three dollars, that had existed some two or three years, and was caused originally by a burn. The man seemed to be about one or two and twenty years of age; and represented his general health, at the time of the operation, to have been very good. He also informed the writer that the ulcer was not painful at the time the limb was amputated, nor had it ever been so after the burn became an ulcer. The case, from the representations of this mutilated son of Africa, seeming to be one contraindicating the sacrifice of the limb so clearly, and consequently exciting the interest of the writer to know more about it, as well as his sympathy, the man was interrogated more fully, especially as to the advisers and operators in the case; to which interrogatories he readily responded, remarking at the same time that he believed that his leg was cut off just to let the students see the operation, and to bring the doctor, as well as the medical college with which he was connected, into notice.

If the representations of this unfortunate man of his case prior to amputation, were correct, the amputation was not only not demanded, but absolutely improper. And if the operation was resorted to for the purposes surmised by the poor sufferer, it places the surgeon before his enlightened and conscientious brethren as a heartless monster. Amputations of the extremities, particularly of the lower, are seldom required in cases of ulcer; and the writer, who claims to have had much experience with these affections, has never yet found it necessary to resort to amputation for their relief in more than one case out of hundreds of instances, and many of them were unpromising and of long standing.

Even in compound fractures, the sacrifice of limbs will seldom be demanded as indispensable to the preservation of life—as in ulcers of the extremities, the writer's experience has been quite extensive, in treating compound fractures of

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every grade of violence, and as yet, after more than twenty-five years' intercourse with them, he has not found it necessary to amputate in a solitary case for their relief.

The sacrifice of a limb is indeed an irreparable loss, and should not be resorted to, unless the compound fracture clearly and unequivocally has virtually severed the member from the body, by destroying the chief arteries that naturally supply it with blood. If one principal artery remains intact, in a large majority of cases the limb can be saved by judicious treatment. But when both or most of the principal trunks are entire, even if the contiguous soft parts have suffered extensively, the chances are enhanced for the preservation of the limb nearly in proportion to the integrity of those vessels, and amputation consequently would be improper.

It is in the treatment of these terrific injuries that modern surgery has achieved some of its most brilliant and important cures; and if surgeons would feel less dread of encountering the troubles and disagreeables incident to the treatment of these injuries, and attach less value to Palmer's patents as substitutes for natural limbs, and place the proper estimate upon those supplied by the creative hand, still farther improvements in this important department of surgery would be made, and fewer limbs lost.

The preceding remarks have been suggested from hearing of several surgical operations that had been performed during the past year most wantonly, in cases utterly hopeless, as well as in others not requiring but actually forbidding such operations, as being needless. They were called forth chiefly, however, by some most cruel amputations performed for the cure of rather intractable but no doubt curable ulcers of the leg, involving the soft parts of the limb only. A limb should never be sacrificed because it has an ulcer on it, unless life would be seriously menaced by a longer continuance of the ulcer, or the ulcer possesses malignant characters; and a surgeon who would thus mutilate needlessly should be mulcted with heavy damages for malpractice.

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ART. IV.—*On the Tabulation of Cases.**—By DR. METHOD, of Virginia.

AFTER a few months' trial of the formulary in the improved form, which I submitted to your inspection, it has occurred to me that I might illustrate its usefulness by giving you a literal copy of the daily records of a few cases. It may be alleged that such a record is kept by many physicians, and the enquiry be urged, what advantage I claim for this over the common diary? I will explain:

In this arrangement, I enter at each visit the name of the person charged, the name of the patient, the services, the fee, and all the particulars of history, symptoms, treatment, &c. in one line. The physician's "*visiting list*" is combined with all needful memoranda of pathology and treatment. The entry is made in surprisingly brief time, and with very little trouble. A friend, who has used the form six months, declares that he would not be without it, and is amazed at the expedition, ease and accuracy with which he puts upon this record all the facts he would desire to remember, whether from visit to visit, or at longer periods.

The heads being ready written, suggest the subjects of comment, and save the labor of writing that number of words.

If I wish to refer to a particular case, I have only to look to my leger as an index, and see the date when I attended such a patient. Turning to this diary, I read its history

* We have received a second letter from an esteemed correspondent, on the important subject which he has so much at heart, viz: some accurate, convenient and portable method by which the physician may note down at the bedside all the facts and symptoms as they present themselves on his daily visit. The readers of the Virginia Medical and Surgical Journal will remember his interesting letter on this subject, with the table explaining his method of annotation, which was published in the number for May 1855, p. 378. The present communication gives a specimen of some cases *taken literally* from his formulary, and we cannot too warmly urge our readers to imitate, in all important cases, this praiseworthy example.—*Editor.*

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faithfully recorded from day to day, according to the impressions each successive observation made upon my mind.

If I wish to see how many cases of obstetrics, or menorrhagia, &c. or pleurisy, or fever, or rheumatism, &c. I may have met with in a year, or a series of years, I inspect the column of "Name of Disease," and I get the information.

Directly associated in the other columns we may see how often and to what degree the leading functions and organs participated in the disorder. The skin, the head, nerves, tongue, stomach, liver, bowels, the respiratory apparatus, the circulatory, genito-urinary, sleep—all are noted, in every case. The remedies and effects of remedies are also exhibited in the same convenient collocation.

This diary being carried in the pocket, the physician is enabled to compare his notes of successive days' observations at the bedside, thus making easier and more accurate each particular record.

It is confidently believed, as I have heretofore said, that the keeping of such a record will tend to make the practitioner reflect better upon the facts while he observes them at the bedside; will lead to more study and investigation of authorities, and especially divest the mind of those thousands of figments which hang about the memory of past experiences; an advantage, however, common to all methods of accurate note keeping.

May 1, Tuesd.—Case I.—Nat, age 5, slave. Date attack, 8 days ago. Skin cool; has cold sweats in evening; tongue a little foul; slimy, dirty, such as often attends "*worms;*" has had no stool since Saturday; respiration not affected; pulse 80; hangs and jerks; strong enough. Prescribe vermifuge aperient. Followed by soup, camphor and ammonia, if pulse fails and cold sweats persist.

The boy required no further attention; many worms passed, and he recovered, as I afterwards learned, looking badly, however.

[It is to be observed that the history of a case like this,

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after it is laid aside, is to be got at by special enquiry of the friends, and a column must be assigned in the "*scheme*" or "formulary" for such statements.]

Aug. 4, Sat.—Was called again to see Nat, (I copy literally fr. the table,) "age 5, slave; date attack Monday last. Visit A. M. Skin hot; head aches; delirious; at times waking; tongue spotted, furred; belly swelled, some diarrhœa; respiration quicker than natural, p. 112; pain in head and belly; starts in sleep, and cries out. Prescribe vermifuge again.

Aug. 6.—Vis. P. M. Skin hot; head still aches; tongue not foul; bowels were moved by the vermifuge, but no worms; resp. quicker than natural; pulse 108; seat of pain, head and bowels; sleep disturbed. Presc. blood cups to belly (ilio cecal valve) and behind ears; ice; cal. ½ gr. ip. ⅛, ev. 4 hours for 3 or 4 days foll'd by teasp. oil and 8 drops turp. spts.

Aug. 8, Wed.—Vis. 7½ A. M. Skin cooler; head still aches, he says; tongue cleaner; bowels moved often yesterday and last night by oil yesterday and powders; resp. less hurried; pulse 100—104; sleeps better. Presc. blist. to belly; repeat cups behind ears; contin. cal. and ipecac. and ice to head.

Aug. 10.—Frid.—Vis. 3½ P. M. Fever less; head aches yet; tongue cleaning; several stools yest. and to-day; some cough now for first time; pulse 100; sleep better. Presc. cups behind ears again, and ice to head; stop powders.

[No further visit paid. Boy convalesced and was well in a week.] Supposed to be case of typhoid fever.

Case II.—June 13, Wednesday morning.—Vis. Lucy, slave, age 10. Attack last Thursday. Skin cool; tongue rather foul; bowels much moved yesterday, quiet to-day; very little cough; physical signs not examined; pulse 116, too strong; seat of pain the belly; give 8 drops turpentine spts. 3 times to-day; hot poultices to belly.

14th, Thursday.—Vis. twice A. M. and P. M. Hotter skin than natural; head too hot; talks in sleep; rational awake; tongue furred in middle, moist; bowels moved yesterday; pain on pressure everywhere; some cough; large mucous râles, and sonorous; p. 116 A. M. 120 P. M. No sleep last night; talks awake and asleep. Presc. blood cups

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in morn. to belly; injection of starch water in large quantity to open the bowels; hot poultices to belly; hot baths to feet; ice to head.

15, Friday.—Vis. A. M. Skin hot; head hot; tongue cleaner; bowels moved twice voluntarily; some cough; p. 104; no pain now in belly; good sleep last nt. no talking. Presc. contin. poultice; cold to head; sponging with cold water, &c.

June 16, Sat.—Vis. A. M. Skin too hot; head do. tongue cleaner; bowels moved twice yesterday voluntarily; coughs very little; p. 110—116; no pain; sleep good last night; no talking. Presc. diet and quinine as soon as fever subsides, which it is said to do every night.

19, Tuesday.—Vis. P. M. Skin too warm; very deaf, suspected to be from quinine; tongue clean; no stool since Sunday; no tenderness on pressure; more cough; no pain in chest; large râles when coughs; rt. and left lower lobes dull; some râles (muc.) and subcrepitus; pulse 116, strong enough; sleep day and night good; said to be better; doubtful! stop quinine, wh. she has taken at periods at night since Sunday. Presc. castor oil ℥i.

June 20th, Wednesday.—Vis. 10 A. M. Skin rather warm; very deaf; tongue clean, moist; bowels moved by oil yest. natural looking stool; some cough; no pain; respiration less frequent; râles less than yesterday; vesicular respiration audible in lower lobes of both lungs; pulse yest. ev'g only 80, is 100 now, (10 A. M.) sleep last nt. very good. Presc. powders of cal. ½ gr. ipec. ½ gr. every 4 hrs. to-day; quin. 2 grs. ev. 2 hrs. when fever goes off.

21, Thurs. (end of second week.)—Vis. 9 A. M. Skin hot; head too warm; tongue clean; no pain in belly on pressure; same cough continues; pulse 120, (was only 100 at 10 A. M. yesterday;) no pain anywhere; sleep good last nt. Presc. castor oil ℥i. Did not take quin. last nt. fever holding on. Order to give it to-night if pulse is less than 100.

22, Friday.—Vis. A. M. Skin too warm; less deaf; tongue a little furred; bowels acted 3 times yesterday; cough yet; pulse 100—104; sleep good last nt. Presc. nothing to-day.

26, Tuesday.—Cough continuing, blood cups were applied few days ago between scapulæ; signs, subcrep. and sibilant râles in both lungs. This day the fever continues strangely, with almost no abatement; pulse day and night is 112—116.

27, Wedn.—Suspect lungs to be cause of this vascular excitement, although it has been supposed heretofore that the signs noted were result only of febrile exacerbations.

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June 27, Wednesday.—Vis. 6 A. M. Skin cooler, tongue not foul; no stool since yesterday; resp. dry, sibilant râles; pulse 112, not so strong; no pain; sleep good. Cups to right side.

29, Friday. Dr. L. called to consult; skin hot, the pulse being 116 and strong; 2 healthy stools to-day; cough unchanged; vesic. resp. audible in all parts of lungs; sleep pretty good. Presc. Norwood's tr. white hellebore 1 part, swt. spts. nitre 3 parts. Dose 10 drps. ev. 3 or 4 ho.

30, Sat. 9 A. M.—Skin cool; no stool to-day; pulse 100; was reduced to 85 in 30 min. after a dose of the hellebore; order hellebore to be contin. or not acc. to circumstances.

July 1, Sund. 7 A. M.—Three stools yest. and to-day; healthy; cough unchanged; pulse 116 yest. in spite of hellebore. It was 100 when I came at 7, but was reduced to 88 in 10 min. by the hellebore—dose 12 drops. Order continue the hellebore, tho' its effect don't seem to be permanent. Suspect *left il. regn.* seat of typhoid fever lesion; apply blister there.

Monday, July 2, 9 P. M.—Skin not hot as usual; tongue furred; blister on belly drew yesterday; pulse 100 or less all day; took 2 doses hellebore to-day. Girl's master thinks it does no permanent good. Discontinue.

Tues. July 3.—Skin cooler at 8 A. M. tongue cleaning; cough continues; pulse 84; master thinks helleb. has had no effect, and dates the improvement fr. the drawing of the blister.

July 4.—Blister renewed.

July 10.—Skin yet too warm; bowels act spontaneously and healthfully; pulse still 116! sleep good; cough gone. She is gaining strength, and I advise to let her alone.

July 17.—Lucy is very well and going about, but her pulse is 120.

This is an example of very many similar cases. It is to be observed in what detail this method of record has presented the history. Each day's note, made at its right time, occupied, perhaps, three minutes, the *heads* being all ready printed.

The story might be abridged, and would be, for ordinary publication.

Some things appear worthy of remark in the case, to wit: The persistent cough, not yielding to any remedies, and

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having exacerbations coinciding with the febrile paroxysms; the effect of the hellebore; the coincidence between its use, the application of a blister to the belly (where no pain with or without pressure was complained of,) and the declension of the pulse for a short time, and *decided convalescence.* But still the pulse resumed its frequency, so that at my last observation, while she was walking about, it counted 120!

Examples of frequent pulse, *continuing* so *after convalescence is established,* are not unusual in our typhoid fever cases. The reverse also, *very slow pulse,* from 30 to 45, is scarcely less frequent, and is most unaccountably and remarkably restricted to the northern portion of this county. Many cases might be recited. The respiration is observed in some cases to continue its morbid frequency, independent of the pulse—that is, bearing no proportion to the pulse. May the reason be found in the state of the organic system of nerves?

I copy another:

Dysentery.

August 9, '55.—M. J. T. female, 14. Poorly for several days; passed some blood from bowels yesterday; lives in a log-house near woods; weeds, &c. very rank around the house and yard; skin rather feverish; several bloody and painful stools in night. Presc. cal. 8 grs. oil 1 table spoonful, to be followed by small doses, 2 grs. of calomel and Dover's powder, if pain and blood persist after action of medicine.

Friday, 10 Aug. 11 A. M.—Some fever; tongue not foul; 4 stools to-day; many yesterday, from the medicine, bloody, &c. pulse too quick; pain in bowels. Presc. cups, poultices, Dover's powders, injections, 15 drops laud. in 2 tablespoonfuls starch water, after every 2 passages, (which I observe as a general rule.)

Sat. 11 Aug.—Has taken 2 Dover's powders to-day with benefit, the injections having failed; has had only 2 stools since 7 A. M. and it is now 6 P. M.

Tuesday, 14.—Has had some symptoms of relapse. Apply cups; give small dose of castor oil, to be followed, when it has operated several times, by Dover's powders 6 or 8 grs. to repeat Dover's powder once in 4 or 6 hours. A blister in in the morn'g, if symptoms persist.

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15th.—The blister has been drawn, and the oil having acted too often, one Dover's powder was given. Bowels have been quiet ever since.

16.—Visit at 7 P. M. Has had 2 healthy stools to-day; pulse 68 or 70; convalescent.

There is nothing remarkable in this case, except what I have noticed in all my experience of dysentery, the *prompt effects of cupping over the bowels, or of cupping conjoined with blisters.*

I have always thought that cups, blisters and injections of starch and laudanum, with mush poultices, were not sufficiently appreciated. At some future time I may furnish you the facts, from my diary, of a great many cases; or rather, some of the facts, since a detail of these might be too voluminous.

It is clear to my mind that in this age, when the minds of men are bending so much to the accumulation of *facts, facts,* and recognize no general law which is not based upon facts, a scheme like this must be of great value to our profession. Who can tell what results might flow from well tabulated experiences, directed by the same principles of observation, recorded in every part of the commonwealth or of the union. Tables, embracing not only the facts immediately connected with disease, but thermometric, barometric, hygrometric conditions. If only one physician in each congressional district would consent to use a pocket diary, so constructed as to exhibit the information referred to, the records might supply, hereafter, to some industrious investigator, materials for invaluable generalization. There would then be some chance for a report upon "medical topography" to our societies.

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ART. V.—*On the Development and Propagation of Cholera.* By DR. HUETTE, Medical Superintendent of Epidemics in the Arondissement of Montargis. Translated from the Archives Génerales de Médecine. November 1855.

WE here approach one of the most serious and difficult questions which cholera has ever suggested—a question which, in a medical aspect, and in its influence on the public health, involves the most important interests of humanity. How is cholera propagated? Is it or is it not contagious? Can medical men decide this point? Does not the government, by its silence, abandon the people to the perilous hazards of a blind confidence in its noncontagion?

The doubt which envelops this sad subject compels a reserve, justified by reasons of the highest philanthropy. But the public safety imposes on us a great duty. The truth must be known; for here, error will be, in the language of a celebrated philosopher, "the enemy of the human race;" but when doubts give way to certainty, the government, instructed and directed by men of science, will be able to take wise and effectual precautions against the future progress of this malady.

We have discussed frequently and written much on the word *contagion*, but its definition still remains undetermined. We do not here design to follow the academies in their controversies; our more modest mission is to examine this important question by a rigorous observation of facts. Seeking then the truth, without holding any preconceived opinions, we have examined the facts coming under our own knowledge, to discover, by a scrupulous study of all the details, how the cholera was developed and propagated in the arondissement of Montargis. If the conclusions to which we are conducted are received with hesitation, we still hope that deductions drawn from more numerous and carefully studied facts, may lead to more satisfactory results. When we attempt to determine the march of a pestilence which

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overleaps oceans with a bound, and pervades the world with the rapidity of a hurricane, we do not expect that the circumscribed area coming under our own knowledge can furnish to the observation the elements of a judgment without an appeal.

Among the localities where we have followed the progress of cholera, some have exhibited the epidemic spontaneously developed, without any known cause and without any connection direct, or indirect, with the infected districts. The cases gradually multiply, and at last the infection becoming general, extends step by step throughout the whole neighborhood or town. This is *spontaneous cholera*, generated under the influence of the epidemic cause, and propagating itself by its own property of expansion.

Other localities enjoy complete immunity, whilst the neighboring countries are cruelly scourged. Preceding epidemics have spared them, and the security derived from the past is strengthened by their freedom from disease at the present. This harmony continues up to the day when some wanderer from an infected locality transports the germ of the disease, and deposits it in the midst of the population heretofore exempt. All at once the pestilence flames out; it strikes first those who live under the same roof with the first victim; it is propagated rapidly from one to another; the voice of mourning is heard through the village, through the city; then it disappears suddenly, like a wandering traveler who has lost his way. Many communities have presented to us the calamitous spectacle of this accidental manifestation of cholera, and we will call this *imported cholera.*

The facts which prove the importation of cholera are of so much importance, that we will relate them with all the details, the authenticity of which we can guarantee; we will then examine the various conditions which favor its transmission after the first importation.

The epidemic of 1854 was developed *by importation* in fourteen communes: St. Maurice, St. Ferrand, Oussoy, Che-

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villion, Corquilleroy, Presnoy, Chatillon, Montcorbon, Amilly, Nogent, Paucourt, Sceaux and Trigueres.

In four communes, the choleraic importation was through the agency of nursing children, a circumstance to which we will allude hereafter more particularly.

St. Maurice-sur-Fessand.—A nurse brought from Paris on the 28th of June an infant, who exhibited the symptoms of cholera on the day of its arrival at St. Maurice, where no case had previously existed. Two days after, the nurse's own child was taken, and died on the 3rd of July. The child from Paris died the next day.

The nurse was seized with cholera on the 4th of July, and was attended by her mother Bernier, who came express from Moulon where there was no cholera. The mother Bernier was attacked the next day with fierce cholera, and succumbed to it in twelve hours. Her eldest daughter, Merlin, also came from Moulon to nurse her mother and sister. Two days after, she was attacked, had the disease severely, but recovered. The father Bernier was also infected, but recovered.

Of the six persons who sickened after the arrival and during the illness of the nursling from Paris, three died. The epidemic remained concentrated in this family; and there was no other case in the village.

Oussoy.—A woman named Bresson, from the hamlet of Moulin-Neuf near Oussoy, where there was no cholera, brought a nursling from Paris on the 27th of June. The day after its arrival the first symptoms of cholera appeared, and it died on the 3rd of July. Some days after, a child of Bresson's was seized and died on the 13th. At the same time the woman herself was struck with the disease, and succumbed on the 17th, after having been nursed by two neighbors, Sahan and Moret. These poor women were taken soon and died—one on the 16th, and the other on the 24th. The husband of Bresson died on the 27th. The woman Burette, who lived at the other extremity of the hamlet, came to wash the clothes of the two women Sahan and Moret, and was seized with the cholera.

The epidemic thus propagated, killed eighteen persons in a short time.

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Before the arrival of the infant from Paris, there had been no case in this village, which had also escaped the previous epidemics of 1832 and 1849. We will call attention to the distribution of this hamlet of Moulin-Neuf, which is composed of ten blocks of buildings separated by considerable distances. Cholera was only found in three of these blocks, occupied 1st, by the Bresson and Sahan families; 2nd, by the Moret family; 3rd, by the Burette family. This third block was at the extreme end of the village. In the other houses, where the inhabitants had no connection with these three families through which we have traced the disease, there was not a single case.

Chevillon.—The epidemic was here imported by an infant in the following manner:

A woman named Deschamp died of cholera at the Montargis hospital on the 22nd of August, leaving a baby aged six weeks. This little orphan was brought to Chevillon, distant from Montargis about two and a half leagues, by a woman called Charvillat. Two days after its arrival the child was seized with cholera, and died in one day.

On the 26th of August, Pierre Charvillat showed the first symptoms of the disease, and died on the 28th, being assiduously nursed by his wife and daughter. On the 27th, his sister-in-law was attacked, and died in one day; and on the 1st of September, Angelique Pepin, a nursling of his wife, about six months old, was taken with cholera.

This whole family occupied one room of a house which was isolated and on the edge of a wood. No other cases were observed in the commune of Chevillon.

Corquilleray—Chatelet. The woman Bourdon died of cholera at Langlèe, leaving a sucking infant, who was taken to the hamlet of Chatelet by a female named Lecerf.

On the morning of its arrival at Chatelet, the child was attacked with cholera, and died. The kind Lecerf, who had taken care of it, and another child whom she had with her, died with the same malady in a few days.

We see here five or six cases of cholera in the same house,

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where the epidemic remained. No other case occurred in the hamlet.

Presnoy.—A young woman named Marvis, living at Presnoy, where there had been no cholera, went to Oussoy with her brother to wait upon her parents who had the disease. On their return home after the death of their parents, they were both seized with cholera, but recovered. The little daughter of Marvis, four years old, who did not go to Oussoy, and a baby at the breast, only a few months old, were attacked after a few days, and both died.

There was no other case seen in the village of Presnoy.

Chatenoy.—Julienne Besnard, a nurse, coming from Paris, had symptoms of cholera on the 12th of September. Her mother died on the 16th; Besnard her husband on the 17th. Julienne, who was first seized, recovered.

All these sick persons lived in one room of a house in the midst of a forest. No other case occurred in the vicinity.

Chatillon.—The first case was observed in the faubourg du Pairault, in a laborer thirty-five years old, named Prochasson, who was attacked immediately on his return from Oussoy, where he had gone to take care of his parents who had the cholera.

The neighbors of Prochasson were quickly infected by him, and the epidemic spread throughout the whole faubourg, where it remained concentrated until the 8th of August. At this time the frightened inhabitants scattered themselves throughout the whole of Chatillon, and the cholera showed itself in all parts of the town—one forty-sixth of the population dying.

Montcorbon.—A woman thirty-five years old, from the hamlet of Meniis, was attacked on her return from Diey (Tonne,) where the cholera raged severely. She believed that she caught the disease by washing the clothes of a woman who died of the epidemic at Diey.

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Three other cases were observed at Montcorbon, after the death of the woman who imported it.

Amilly.—A laborer named Arsonneau fell sick with cholera at Buges. They carried him home, two leagues, to the hamlet of Génetois, where the epidemic had never raged. Arsonneau was nursed by his wife. He died on the fifth day. The wife, the mother and the children of Arsonneau had cholera, and one child died. They all lived in the same room.

There was no other case in the village of Génetois.

Saint Germain.—The woman Devin, forty-two years old, left the village of Pouparderie, where there was no cholera, to nurse her sister Guillon, living in St. Germain, where there was spontaneous cholera. After the death of Guillon, she went to her mother at Pouparderie, and some hours after her return she had symptoms of cholera. She recovered, but her mother was taken the next day, and died during the night.

Trigueres.—A woman, Durand by name, aged twenty-seven years, a servant at la Tuilerée, near Chateau-Renard, showed the symptoms of cholera on the morning of the 28th of August. She at once left the house where she lived, and fled to Trigueres, about four kilometres from la Tuilerée, to the house of a friend, where her daughter lived, about four years old. Durand and her child slept in the same bed. During the night the disease increased in violence, and in the morning Durand died. Two days after, her little girl sickened and died. There were no other cases in Trigueres.

After giving these instances of importation, followed by a development of the disease by transmission, we will report some instances of importation by old persons, but without transmission.

At Nogent, we observed one case of an elderly man who had gone to Rogny to nurse his parents. The day after his return to Nogent this man was taken and died. There was no other case at Nogent.

At Paucourt.—A man and his wife, Poget by name, each more than sixty years old, left this village, where there was no cholera, to give their attention to their children, sick at

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Montargis, distant from Paucourt about six kilometres. After the death of their children, they returned to Paucourt; and the day after their return they were both attacked by cholera, and both died. There were no other cases at Paucourt.

At Sceaux.—Forneau and his wife left home, where no cholera existed, to nurse their children, lying ill at Flagy (Seine et Marne.) After their children died, they came back to Sceaux. Both had the cholera and died. There were no other cases in the commune.

Such are the facts which seem to militate in favor of the doctrine of the propagation of cholera by importation and transmission. We might have collected a much larger number, but we have especially selected those which were entirely disengaged from the general epidemic influence, and which might fairly be regarded as not of spontaneous origin. In a populous city, where, in a few days, the disease appears spontaneously at many points at once, it is extremely difficult to separate the cases due to the epidemic influence, and those due to transmission. In the country, however, the study of this problem is much easier. In following the disease from village to village, from hamlet to hamlet, the physician, observing it in its nascent state, can study the law which controls its development, and being freed from all accidental complications, can watch its habitudes in a way which would be impossible to do in the large cities.

Period of incubation.—All the observations of the importation of cholera reveal a phenomenon well worthy of attention—that is, the period of incubation of cholera.

Thus, of the individuals leaving their homes where no cholera existed to nurse their sick parents in an infected locality, we see that they are not seized with the disease at once; they are spared up to the moment of their departure, even until they again reach home. We must surely conclude that they had absorbed the choleriginous miasm at the infected spot. Like all the diseases resulting from a miasmatic poisoning, cholera presents a period of incubation of variable duration, during which time the general health does

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not seem to be disturbed. Nothing indicates the profound perturbation up to the day when a sudden explosion of the characteristic symptoms reveals the nature and gravity of the malady.

This doctrine, so important in a prophylactic point of view, is clearly demonstrated in all the cases which we have traced out in our essay.

If our remedies for confirmed cholera still remain to a great degree powerless, we may yet hope to make some progress in its prophylaxy; and hence the facts we have reported are worthy of the physician's notice, and are of a nature to encourage future researches.

On the transmission of cholera.—The choleraic transmission presents one of the most obscure problems in the history of cholera—one which neither observation or experience has yet resolved. By induction we have been led to many hypotheses, and amongst the many explanations which have been given of this phenomenon, we will adopt that which seems most in harmony with what we have ourselves observed.

This agent modifies the air with which it mingles, producing new elements; morbid products, susceptible, after a longer or shorter period of incubation, of engendering the disease in those who absorb it through the lungs or by the skin. The choleraic secretions and excretions can also carry the germ of the malady.

We have always found the most evident instances of transmission, both in the city and country, occurring amongst the poorer class of the community, where families were crowded into one room, probably a basement, damp and unpaved; where the matters vomited and the alvine dejections covered the ground and the bed clothes. In these deplorable hygienic conditions, whole families are exposed night and day to the pestilential emanations from the sick. Then those who are endowed with a strong organization struggle victoriously against the poisonous influence; but those on the contrary, of defective nutrition, extreme debility, when the

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functions of absorption are more active from an insufficient nutrition, and all the doors of the economy open to these exterior influences, sink in the contest with the morbid element.

The air is not the only agent which transmits cholera. The cutaneous absorption is as dangerous as pulmonary absorption.

The facts we observed, especially at Oussoy and Montauban, proved its transmission by means of clothing and bed linen stained with the cholera discharges. Similar facts attracted the attention of the academy in 1849. In 1850, M. Pellarin related to the academy of medicine a case of cholera thus transmitted after more than a month's interval.

Under these circumstances we must seek to explain the phenomenon of transmission by a fermentation of the cholera vomit and stool, which after a certain time give birth to a cryptogamous production which engenders cholera. This new product as yet escapes our observation. Nevertheless, we are led by induction to those prophylactic applications which are capable of neutralizing the transmitting power of cholera. The use of chloride of lime at Montargis, where we could easily and quickly procure this substance, seemed always to coincide with those cases which remained isolated—so we would advise:

1st. To put the patient in a large chamber, where fresh, pure air may pass in freely.

2nd. To receive the matters vomited and the dejections from the bowels into vessels containing a solution of the chloride of lime.

3rd. To bury these matters immediately, at a certain distance from the house.

4th. To plunge the soiled clothes into tubs containing chlorine water.

5th. The free use of the chlorides in the apartment.

6th. Two frictions a day with chlorine water, on the body of the patient.

By these means we *may* diminish the risk of transmitting cholera.

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On transmission considered with regard to age.—Another fact worthy of remark, and which our observations bring out in strong relief, is that the ravages produced by *imported cholera* are, with some exceptions, in an inverse proportion to the ages of the persons who are the points from whence the infection emanates. When cholera is carried by old persons, the cases are less numerous, and often limited to those first attacked, as we observed at Paucourt, Nogent and Sceaux. When cholera is imported by adults, its propagation is more evident; when infants bring the epidemic, the transmission is rapid, and the effects deadly. Is the power of infection and transmission increased by the organic power inherent in infancy, just as a graft flourishes more or less vigorously, depending upon the stock upon which it had been placed? We are inclined to believe so, and are the more confirmed in this opinion by the investigations of M. Diday on phenomena of a similar character—that infants transmit secondary syphilis more rapidly and easily than adults.

On the development of cholera considered in connection with geology. Chart of spontaneous and imported cholera.—If the appearance of imported cholera at different points is entirely subordinate to the accidental influence of infected individuals, it is not the case with spontaneous cholera. Whilst the causes which give birth to the disease are as yet inaccessible to our means of observation, we may presume, *a priori*, that this spontaneous appearance, like all the phenomena of nature, is under the guidance of immediate laws, the mystery of which may sooner or later be penetrated by the human intellect.

If the two varieties of cholera are admitted, we can understand how important it is to study the march of the epidemic in connection with the geological constitution of the earth. By separating the numerous instances due to importation, we may follow up this pestilence to its birth place, study the telluric and atmospheric conditions which coincide with its appearance and favor its development, and conse-

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quently the better indicate a port of safety in the time of danger.

One word on the geology of the arondissement of Montargis. This district is a vast basin, washed on the north and south by the river Loing and the canals of Briare and Loing, passing through an alluvial soil, and in the centre of large plains. All the other water courses, the Clery, the Vernisson, the Vesine, &c. empty into the great central artery, the Loing, which divides the basin into two equal parts. The most elevated parts of the district occupy then the periphery, and slope gradually towards the valley of the Loing. The peaks and hillocks surround at some distance the plains and water courses. There is a good deal of forest country in the northeast; to the east the country is level, with a chalky or marshy soil, and an argillaceous subsoil.

In following the advance of the epidemic of 1854 on the map of the arondissement, we observed that spontaneous cholera was always found on the low grounds of the Loing and its tributaries.

As we left the banks of the rivers more and more, spontaneous cholera became more rare, less deadly, and at last completely disappeared. The epidemic concentrated its ravages on the great water courses, and more particularly in the centre of the basin of the canton of Montargis. Around this basin cholera only appeared by importation, and beyond the zone of importation the country was spared.

It was then on the alluvial bottoms of the Loing that the disease raged spontaneously, whilst the chalky and marshy lands occupying the periphery, always the favorite seat of intermittent fevers, enjoyed complete immunity.

We regret that we do not possess any document on this important question during the epidemics of 1832 and 1849. It would have been useful to compare the three epidemics with the topography of their development.

We will close by expressing the wish that the epidemiographers, who are now examining this subject, would prepare in each district a chart of spontaneous and imported cholera,

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which would be of great value in case we should have another epidemic.

We then maintain that cholera is developed in three ways:

1st. *Spontaneously*, by virtue of causes which are unknown to us and by an expansive force which is proper to itself.

2nd. By *importation*, or by the migration of infected individuals into healthy localities.

3rd. By *transmission*, or by the force of expansion proper to the disease after the first fact of importation.

II. The aptitude to contract the disease in the focuses of infection is the same at all ages.

III. Infancy increases the expansive force of cholera and facilitates its transmission.

IV. There is in cholera, as in other diseases, a period of incubation of variable duration, (from forty-eight hours to eight days,) during which the disease exists in a latent state until the explosion takes place.

V. Certain geological conditions seem more or less favorable to the development of spontaneous cholera, or to weaken its power of transmission.

VI. The *diarrhœa* which precedes cholera, perhaps favors its development—1st, by weakening the subject; 2nd, by exciting the functions of absorption. But we do not consent to the name of *prodromic diarrhœa*, which establishes between it and true cholera a forced correlation. The majority of facts seem to prove that this is merely a coincidence. In the caoutchouc manufactory of Langlée, where we had the supervision of more than 600 workmen during the epidemic, we noted 400 cases of diarrhœa. About 150 were treated by us and followed our prescriptions; the others persisted in the use of the most unhealthy and dangerous diet and habits; yet we only saw 15 cases of cholera.

VII. Our prognosis of cholera does not depend on the form or intensity of the symptoms. It depends on the time when the disease made its attack. An epidemic, fatal at

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one time and mild at another, will present the same symptoms. The most alarming choleras terminate frequently in unexpected recoveries, and the mildest, apparently, will end fatally. There is a mysterious cause which we only know by its effects, and we can only suppose that the choliferous miasm in the one case is discharged in fatal doses and in the other in doses which will not kill.

VIII. Cholera, having the property of spontaneous development, defies all prudential measures. Quarantines and cordons sanitaires, designed to arrest its progress, have often no other result than to hamper mercantile transactions and impede personal liberty, whilst by exciting terror in the minds of the people, they are prevented from meeting the pestilence with that resignation with which we should always bear an inevitable calamity.

LECTURES AND SELECTIONS.

I. *Lecture on the Lamina Spiralis of the Cochlea.* By JOHN GOODSIR, F. R. SS. L. and E. Professor of Anatomy. Delivered in the University of Edinburgh. July 1855.

After briefly alluding to the general conformation and relations of the cochlea, the lecturer proceeded to describe the remarkable structures which have lately been discovered in the lamina spiralis.

The lamina spiralis, instead of being, as hitherto supposed, a single layer, osseous in the inner, and membranous in the outer portion of its extent, is a double structure, with numerous complex arrangements in its interior.

The osseous and membranous portions of the lamina spiralis as hitherto understood, may be considered as the basis of the entire complex structure as it is now ascertained.

The osseous portion of the lamina contains the cochlear nerves in closely arranged canals, which, at its outer margin, coalesce in a chink or fissure, which contains the ganglion recently discovered by Corti, and affords exit to the nervous filaments.

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The membranous portion of the lamina consists, as discovered by Todd and Bowman, of a membrane which, except at its outer and inner margins, is closely streaked in the direction of the radius of the cochlea, and hence denominated zona pectinata. The outer margin of the membrane is attached by means of a fibro-nucleated texture to the accessory spiral lamina of Huschke, and to the neighboring groove. This fibro-nucleated structure is the cochlear muscle of Todd and Bowman—the spiral cochlear ligament of Kölliker. The inner margin of the membrane is attached to that lip of the fissure of the osseous lamina which is next to the apex of the cochlea, but so as to leave numerous orifices or more or less oblique canals, through which, as Kölliker has ascertained, the extremities of the cochlear nerve filaments pass to the vestibular surface of the membrane. The cochlear nerves, therefore, instead of being distributed, as has hitherto been supposed, on the tympanic aspect of the lamina spiralis, pass through it to its vestibular aspect; its entire tympanic surface, and the nerves in their transit across that surface, from the fissure in the osseous to the orifices in the membranous portion, being covered by the fibro-serous lining membrane of the osseous labyrinth. It thus appears that all the complex structures in connection with the lamina spiralis, usually so called, of the cochlea, are situated on its vestibular aspect.

These structures are: 1. The habenula sulcata, situated chiefly on the osseous lamina, and discovered by Todd and Bowman; 2. The habenula denticulata, situated on the membranous lamina, discovered by Corti, and latterly ascertained by Kölliker to be connected with the cochlear nerves; 3. The membrane of Corti, covered on its vestibular surface by the serous lining membrane of the osseous labyrinth, first partially recognized by Corti, but latterly more fully described by Claudius; 4. Large vesicular cells which occupy the space between the membrane of Corti, and the lamina spiralis, usually so called, first recognized by Corti, but more precisely determined by Claudius.

The habenula sulcata is a structure of cartilaginous aspect, which, rapidly increasing in thickness, as it advances to the outer margin of the osseous lamina, inclines over and beyond that margin so as to form the sulcus spiralis of Huschke. It consists of columns, which at its thin inner edge are set perpendicular to the surface of the subjacent bones, and therefore expose their free extremities on its vestibular aspect. Towards its thick or outer edge the columns become more and more inclined, so as to expose more and

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more of their sides, and at the edge itself they form a series of elongated, slightly clavate and flattened, clear glistening teeth, which project over the groove of Huschke. Some of the columns divide, others unite together, as they pass outwards to form these teeth; which are termed "teeth of the first series." In the grooves between the columns are numerous nuclei, which resist acetic acid, while the former swell up, and become somewhat striated under its action.

The habenula denticulata consists of a series of apparently jointed rods, laid on the surface of the membranous portion of the so-called lamina spiralis; each rod in the series lying in the direction of the radius of the cochlea; the first or inner segments of these rods form a series of compressed laminæ, attached across the bottom of Huschke's spiral groove, with narrow chinks between them, which are, in fact, the orifices through which the cochlear nerve filaments pass as already stated. These central segments are the "Dents apparents of Corti." The second segment, or portion of each rod, is also compressed, and lies flat on the membranous spiral lamina, and is loose and movable, except at its inner end, where it presents an enlargement like a nucleus, which was supposed by Corti to be the joint by means of which the rod is attached to, and moves on the membrane; but which Kölliker has discovered to be the point at which one or more of the ultimate filaments of the cochlear nerves become connected with the rod, after they have passed through the orifices already mentioned. The series formed by these second segments of the rods are the so called "teeth of the second order." The terminal segment is connected with the second by means of two short quadrilateral segments, the "coins articulaires" of Corti. The terminal segment is elongated and compressed. To its upper surface three pyriform bodies, each of which contains a nucleus, are attached by short peduncles, so as to be laid over one another, from within outwards. Corti and Kölliker describe the outer extremity of this segment as somewhat expanded and forked, and also as free or loose. But Claudius has ascertained that the expanded extremity is attached to the lamina pectinata, so that the rod cannot move to and from the membrane, as Corti has supposed.

It must here be observed, that the first or inner segments of the rods—the "dents apparents" are attached throughout, and from their position and relations belong rather to the structure of the habenula sulcata, than to that of the habenula denticulata. The latter consists, then, essentially of the second and terminal segment described above, connected to-

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gether by the short "articular portions. The cochlear rod, properly so called, consists therefore of two principal segments, the inner segment being connected with one or more ultimate nerve filaments, and the outer fastened at its external end to the membrane on which it lies.

Corti has described a thin membrane covered by the epithelium of the labyrinth, and extending from the prominent surface of the habenula sulcata somewhat beyond the habenula denticulata. The space between this membrane and the "dents apparents," and also the spiral groove of Huschke are occupied, according to him, by large transparent nucleated vesicles. Similar vesicles, conceived to be epithelial by their discoverer, occupy the space between the membranous lamina spiralis and that portion of Corti's membrane which extends beyond the habenula denticulata. The rods of the cochlea, therefore, according to Corti, move like a series of hammer or piano forte keys, in a space included between the membrane discovered by him, and the lamina spiralis, usually so called. Claudius, however, has shown, that Corti's membrane extends out to the external wall of the cochlea; and that the entire space between it and the lamina spiralis, usually so called, is occupied by vesicular structure.

The lecturer now proceeded to state the results of the observations which he had made for the purpose of verifying the descriptions of Corti, Kölliker and Claudius. The membrane of Corti extends, as Claudius has stated, to the outer wall of the cochlea, and the large cells between it and the lamina spiralis have extremely thin walls, and transparent contents, so that they resemble, when pressed together, an areolor net work. Portions of the rods were also occasionally seen curled up, and as observed by Claudius, attached by their expanded extremities, in series, to the membrane on which they lie. The detection by Kölliker of the connection of the nerve filaments with the rods is, after the discoveries of Corti, the most important recent addition to our knowledge of the structure of the cochlea. Even in the ordinary view of the structure from the vestibular aspect, the nerve filaments may, without much difficulty, be observed to disappear at or in the central extremities of the rods. Their inner extremities do not correspond, as Kölliker has correctly observed, to the outer ends of the "dents apparents," but to the intervals between the latter. Kölliker holds, on chemical as well as anatomical grounds, that the rods of Corti are true terminations of the cochlear nerves—peculiar forms of the ultimate nerve fibre. He

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believes Corti's opinion to be erroneous, that they are developments from the membrane on which they lie, and that they constitute a physical apparatus. Notwithstanding Kölliker's opinion, it may be safely asserted, that the rods of Corti present a configuration and aspect which distinguish them in the most marked manner from any form of the nerve filament. The peculiar flattened articulated form, the variable breadth, and, as pointed out by Claudius, the alternate arrangement of the proximal and distal segments of neighboring rods; and, lastly, the elasticity, slight, though marked, which they possess, indicate that they are not true nerve structures. Without stating the nature of the function he supposes them to perform, Corti believes the rods more like a series of hammers. Harless conceives that they act as dampers by pressing on the membrane during vibration. Kölliker, again, believes that from the almost mathematical regularity with which they are arranged along the vestibular surface of the lamina spiralis, these peculiar terminations of the cochlear nerves are the structures which distinguish the pitch, timbre and strength of sounds, through the medium of the water of the labyrinth and the fenestra ovalis.

The lecture concluded with some remarks on the probable functions of the different parts of the labyrinth, and more particularly of the rods of Corti. From comparative anatomy it would appear that the vestibule is that part of the organ by means of which any sound or series, or combination of sounds is heard merely as *noise*. The simplest form of ear, which consists of a vestibule only, probably enables the sensorium merely to become cognizant of sound, irrespective of the pitch, or harmony of its constituent tones.

In regard to the semicircular canals, it appears probable from their intimate connection with the vestibule, that they, like it, have to do with sound merely as *noise*, and that their function, therefore, is of secondary importance in the higher forms of the organ.

Dr. Thomas Young, with his usual sagacity, considered the cochlea as a "micrometer of sound." Kölliker, as already stated, has put forward a similar idea, based on his knowledge of the structures described in this lecture. His conception, however, appears to be so far unsatisfactory, in as much as he considers the rods of Corti to be merely the extremities of the cochlear nerves; and it wants that completeness which it would have, had he been able to admit those rods to be a series of acoustic arrangements, as they are believed to be by their discoverer.

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The hypothesis presents a more satisfactory form if we assume that each of the rods of Corti, or that groups of these rods are so organized and arranged as to act or vibrate as acoustic apparatuses appended to the extremities of the cochlear nerves. Each rod, or group of rods, may be so constituted as alone, among all the others, to act or vibrate, when the note or harmonic chord, for which that single rod, or that group of rods, had been provided, passes through the cochlea in the form of sonant vibrations of a correspondent physical value. If this be the case, we can understand how, by the instrumentality of a cochlea, the physical value of each tone, or harmonic combination of tones, may be detected by the ear, and impressions of correspondent value transmitted along the nerve filaments to the seat of sonant sensation in the brain. It must be borne in mind, however, that the æsthetic perception of the sensations produced by the instrumentality of the cochlea, its nerves, and the sentient centre, is a psychial function, and a result of the pre-established harmony between the mental and corporeal elements of the animal constitution, on the one hand, and external nature, on the other.—*Edinburgh Med. Journal.*

II. *Lectures on Varicella, Variola, and Variolous Diseases.* By Armand Trousseau, Professor of Clinical Medicine to the Faculty of Paris, at Hôtel-Dieu, &c. &c. [Translated from l'Union Médicale.]

Lecture I.—Varicella.

In observing the large number of patients that have recently entered our wards affected with variola or varioloid, you may have conceived some doubts as to the importance of vaccinia, some incredulity as to the protective influence of this potent modifier; or else, perhaps, you have entertained suspicions as to the legitimacy of the eruptions we have treated as varioloid and small-pox, some of which have presented such appearances as might be attributed to eruptions of a different kind. I propose, therefore, to examine, in a series of lectures, varicella, varioloid, variola, and vaccinia, and, in treating of the latter, to describe its origin, its history, and its transformations, and to discuss the subject of revaccination and variolous inoculation, questions which are mooted already, and which are destined to arouse, before twenty-five years have elapsed, the most lively interest and

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the most ardent controversies in the medical generation which will succeed our own.

Before speaking of varicella, which will be the subject of my first lesson, let me refer to some indispensable preliminary considerations, without which my future observations would be unintelligible.

In reading the old authors, and especially that magnificent book in which Sydenham describes the epidemics of small-pox which occurred in his day, you often meet with the words *variolæ anomalæ.* And in the works of later authors, in De Haën, in Borsieri, you still meet with accounts of cases of anomalous small-pox, designated *variolæ anomalæ, irregulares.* As you approach the era of the discovery of vaccination, you encounter, besides these *variolæ anomelæ, irregulares,* other varieties, such as *variolæ cristallinæ, variolæ cornatæ,* that is, crystaline and hornpock.

Before vaccination was known, these forms of variola were anomalous and exceptional. These exceptions have become, in a manner, the rule in our day, a result which depends no doubt on the beneficent influence of vaccinia, which, thanks to an ardent and enlightened zeal, has been propagated everywhere.

Let us enquire, for a moment, into the influence which viruses in general, and the variolous virus in particular, exercises on the economy, when introduced into the human body, in some mode or other, by inoculation or by contagion. The variolous virus, when applied to the economy, produces certain characteristic phenomena. In the first place, an intense fever arises, which lasts a certain period, during the incubation, while the virus lies dormant, or rather undergoes an elaboration. Secondly, there follows an eruption, which I shall describe with care hereafter. For the moment, I desire to put in evidence a fact of capital importance, which is this, that the organism of an individual who has once been subjected to the influence of the variolous virus undergoes a modification, in virtue of which he ceases to be liable to it thereafter. This is true not only of small-pox, but also of yellow fever, whooping cough and other diseases utterly unconnected with the family of eruptive disorders. It is certain that, in all these cases, the economy does not receive a second time, at any rate willingly, the effect of the morbid agent. Nevertheless, it undergoes, exceptionally it is true, but still it sometimes undergoes a second attack. In the histories of the epidemics of the last century, we find authentic cases of small-pox recurring in the same individual; and I have myself seen, in 1831, a student of medi-

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cine, who, after *three* variolous eruptions, was stricken, while under my care, by a *fourth* attack.

It is certain, then, that before the discovery of vaccination, instances were observed of small-pox recurring more than once, and each time with particular modifications. Towards the close of the last century, there was in London an hospital, which still exists, devoted exclusively to small-pox patients, into which all persons affected with that malady were compelled to enter, that the healthy portion of the population might be protected from contagion. Here, numerous examples were seen of small-pox in persons who had once had the disease naturally, or who had been inoculated with the variolous virus. But it was also observed, and recorded positively, that these secondary eruptions were unlike the primary ones; that the pustules were less prominent, less numerous, that they were horny; in a word, that these were cases of modified small-pox.

In our day, after the population of Europe had been preserved almost completely, by vaccination from epidemics of small-pox, up to the year 1820, epidemics occurred, at Marseilles, at Edinburgh, and at various localities in England. In the course of these epidemics, a large number of persons who had been vaccinated were observed among the patients—four or five thousand such cases at least were observed. At this period, a peculiar form of variola was described, to which the name *varioloid* was given. Persons who had once had small-pox had varioloid as well as those who had been vaccinated.

These facts caused medical men to reflect. Some physicians expressed doubts as to the legitimacy of those attacks of variola which had not rendered the persons who had suffered from them refractory to the influence of vaccinia, nor protected them against a second attack of small-pox. They finally concluded that the pretended antecedent variola was nothing but a simple varicella. They thus separated varicella from variola, whereas the majority of physicians, who always admit with reluctance that a disease is of a specific nature, denied that there was any specific difference between small-pox and varicella, and pronounced the latter a peculiar modification of the former.

Having premised thus much, let us pass in review the characteristics of varicella, dwelling more particularly on those which distinguish it from variola or varioloid.

Varicella, petite vérole volante, cristalli, chicken-pox, swine-pox, is a disease which usually attacks young children, and very rarely adults. Varioloid, on the other hand, attacks

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young children who have been vaccinated, almost never, and prevails chiefly among adults. Here is a good differential characteristic.

Whether an individual has been vaccinated or not, whether he has had small-pox or not, the phenomena of varicella are always the same. The disease is not modified, but presents itself in all individuals in whom it is developed, with its peculiar fever and eruption. It commences with an active febrile movement, without vomiting or pain in the back; whereas in variola and varioloid, vomiting and lumbar pains are almost constant precursors of the eruption. Before twenty-four hours have elapsed, sometimes in four or five hours, as in scarlatina, little red papules appear, rising slightly above the level of the skin, and resembling the small-pox pimples at the second day. The initial fever is of such short duration, that the physician rarely observes it at all. Such a phenomenon occurs in no other pustular disease.

The eruption once out, the fever continues for four or five days, but still presenting a peculiarity. It disappears almost, during the day, and lights up at night. Each exacerbation of fever is followed by a new eruption, so that if, we will suppose, you have counted 20 pimples, on the face the first day, you will see 30 on the second, 40 on the third, and 50, 60, 70 on the succeeding days; it will be the same on the trunk. Thus, instead of exhausting itself like variola, rubeola, and scarlatina, in a single effort, and throwing out everything on the skin in twenty-four hours, varicella pays in small change, so to speak. The eruption comes in groups on different days, so that a medical man, in the habit of seeing varicella, has no difficulty in distinguishing the eruption of to-day from that of yesterday and the day before.

The form of the pustules of varicella is so utterly unlike that of variola, that it suffices to see them once never to confound them. The pimple of varicella is red and pointed. It is converted, in twelve or twenty-four hours, into a vesicle or bleb resembling the bulla of pemphigus, of one form of erysipelas, of some varieties of herpes, and especially of *herpes zoster or the shingles.* This vesicle or bulla is isolated, rounded like the blister produced by a drop of heated oil or water; it is distended, perfectly limpid, and contains serum instead of pus. Fifteen hours after it forms, a little inflammatory areola, much more precocious than that of variola, appears around it. If you stretch the skin on which the areola is located, it disappears, and the transparent bulla remains, like a drop of limpid water. The next day,

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the bleb enlarges and becomes deformed, losing its roundness and its transparency; the liquid becomes opaline. Twenty-four hours subsequently, the inflammatory circle is observed to be larger, and of a deeper red than the analogous areola of small-pox. Instead of that agreeable rose-tint peculiar to the areola of small-pox, *læte rosea*, according to Sydenham's graceful expression, it assumes a livid red hue. On the fourth day, the bulla dries, and leaves an irregular black crust like the scab of a pustule of ecthyma.

When the last pustule is out, the fever abates at once, unlike what occurs in variola or varioloid, in which a fever of maturation or suppuration sets up on the eighth day of the disease. There is no fever in varicella after the eruption is once *out*, although the surface may be covered with pustules. Usually, the bullæ are dry by the fifth day, and the disease is over. Occasionally, however, the formation of bullæ continues for a time. I remember to have seen, during an epidemic of varicella I observed at the hospital Necker in my children's wards, cases in which the eruption continued for two or three weeks. Towards the close of the disease, in these cases, the bullæ assumed the appearance of pemphigus, and grave constitutional symptoms appeared. But there were exceptions. Since I have practiced medicine, I have never seen, either in my private practice, nor in the children's hospital, to which I was attached for fifteen years—I have never seen, I say, a single death from varicella.

Varicella follows absolutely the same course in those who are vaccinated and those who are not. I have known varicella at the hospital Necker, where I had only very young children, all of them vaccinated, to attack every child in the house. Now the immunity from small-pox afforded by vaccination is sufficiently protracted to make it altogether exceptional to observe small-pox in vaccinated children, whence it is necessary to conclude that varicella is not a simple modification of variola. As a counter proof, we see that when the preservative influence of vaccinia fails, children are attacked by variola, not by varioloid. Now varicella resembles varioloid much more than it does variola. If, then, varicella was a mere modification of variola, we should not expect vaccinated children to be affected by it. You see vaccinated children take small-pox, and afterwards varicella, as readily as they take measles, or scarlet fever, or erythema modosum, or any skin disease.

There is an essential difference, then, between varicella and variola. The progress and duration of the initial fever, and the progress and duration of the eruption, are charac-

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teristics distinguishing varicella from all other eruptive diseases.

Varicella, though wonderfully benign, since I have never known a child to die of it, leaves traces on the skin, nevertheless. These marks may be mistaken, and are so, by parents, who are excusable, and by physicians, who are not, for marks of small-pox. Generally, parents do not err as to the nature of the eruption, and often when a physician, less experienced in the matter than they frequently, declares that the child has small-pox, the mother exclaims: Oh, no, my dear sir! are you not mistaken? this is only chicken-pox, not small-pox. Their mothers know well enough that varicella and variola are not the same things. Yet there are parents, who have dabbled a little in physic, who believe in their identity, and reply, when advised to have their children vaccinated, Oh, it is not worth while; they have had the *picote*, the modified small-pox. Now these pretended small-pox patients, where an epidemic of variola occurs, contract the disease, which runs its course in them without the slightest modification; and, if you vaccinate them, the vaccine pustule proceeds normally. Then, some physicians, armed with these facts, exclaim, You see that persons may have small-pox twice, and may have vaccinia after small-pox! Undoubtedly, this may occur, but it is very certain that the majority of those cited as examples of cases of secondary small-pox, had only a varicella in the first place; and this error arises from the fact that many physicians do not know how to distinguish varicella from variola, although, as we have shown, the thing is easy beyond expression.*

* It might seem to many of our readers that M. Trousseau was arguing against a man of straw, since few of our physicians confound variola and varicella. Not so in France, where the identity of these diseases is stoutly maintained. Thus Valleix, whose work is an authority in France, says, (GUIDE, p. 428, vol. v,) "The facts adduced by Thomson, Bérard and Délavit, appear to me to remove every reasonable doubt in regard to varicella being other than a modified small-pox; and the proofs which daily accumulate in support of this view are so convincing that the arguments of objectors have almost lost all interest." For all this, we think it very likely that the learned and admirable author never observed a case of varicella in his life, now, alas! prematurely ended.—*Editor.*

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III. *Memoir of Magendie.*

François Magendie was born at Bordeaux, (department of the Gironde,) on the 6th of October 1783. His father, who was a physician of some talent, resided in Paris, where the young child was brought soon after his birth. The early days of the future great man passed without any important event—with the exception of the death of his mother. His father was soon married again to a woman who took little interest in her husband's child. The great French revolution came, and all the ardent men of the time had a share in it. This was the case with Magendie's father, who; devoting all his time to political affairs, neglected his son. This enthusiastic politician became successively mayor of the 10th arondissement, a member of the hospital council, and also of the celebrated commune de Paris.

The young Magendie did not learn to read or write until he was 10 years old. So rapid then was his progress at school, that when only 14 he succeeded in gaining a prize in the annual concours, where all the best scholars of Paris compete. The Latin and Greek languages were not taught in the schools in those days, and Magendie studied them afterwards by himself, when a student of medicine. His father was a friend of the celebrated Surgeon Boyer, and when only 16, Magendie became a private pupil, and soon afterwards the prosector and demonstrator of anatomy under Boyer. When 20 years old he was admitted (after a brilliant concours) as (Aide d' Anatomie) assistant demonstrator of the faculty of medicine, and very soon a prosector (demonstrator.)

Magendie began when very young to deliver successful lectures on anatomy and operations. At that time he intended to become a surgeon, and was about presenting himself at a concours, for a place as surgeon at the general board of the Paris hospital, when Dupuytren, unwilling to have such a rival, induced him to abandon this branch of the healing art—and it is owing to this that Magendie, who naturally had strong predilections for surgery, became a physician.

His success was rapid, and he was soon admitted a member of many scientific and medical societies. His reputation as a physiologist was already very great. Some of his most important papers had already been published, and had received the sanction of the Academy of Sciences. In 1819 he became a member of the Academy of Medicine, and, two

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years after, with the assistance of the great astronomer, De Laplace, he was elected a member of the Academy of Sciences, at an age when few men are admitted into that learned body, in which there are only six places for physicians and surgeons. He had just begun the publication of his important "Journal of Physiology," which has been so useful to the progress of science.

In 1831, Récamier, not willing to take the oath to the king, Louis Philippe, was compelled to resign his professorship at the College of France, and Magendie was elected in his place. From that time almost until the day of his death (7th of October last) his activity—as a member of the Academy of Sciences, as a professor in the first of the scientific schools of France, and as a member of many committees appointed by the government, and, particularly, the committee of public hygiene—had given him an unbounded influence, which he always employed for the benefit of science.

The discoveries of Magendie are not so well known as they ought to be. A short glance at some of them, and particularly at those which are somewhat forgotten, will show how much physiology is indebted to him.

Among his most important papers are those relative to the power of absorption of the veins. The experiments of John Hunter, Cruikshank, Makagni, Desgenettes, and others, had been admitted as proving that lymphatics alone have the power and the function of absorption. By admirably conceived experiments Magendie clearly proved that the veins also have this power and this function.

In experimenting upon the absorption of poisons, Magendie found a fact which he thought to be very strange. It is, that in a poisoned animal, the blood, although containing the poison, and being mostly the agent by which the toxic substance is brought to all the parts of the body, is not able to poison another animal when it is injected into its blood vessels. This result, which resembles very much some of the results of experiments of Dr. Addison and Mr. Morgan, may, I believe, be easily explained. The dose of the poison in the blood injected into the second animal was not large enough. Dr. Brown-Séquard has seen poisoning quickly produced by injecting into the femoral vein of an animal, blood which had been drawn from the veins of a large poisoned animal.

The researches of Magendie on the mode of action of different substances containing strychnia, are very important. Not only the symptoms which are to be observed in animals

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poisoned by these substances were carefully studied, but Magendie found many interesting circumstances relative to the mode of action of these poisons, and pointed out a fact which may prove useful in the treatment of poisoning by strychnia. He showed that these substances do not act directly on the brain, and that their principal action is on the spinal cord, and he showed that it is by asphyxia that they produce death. He observed that fits were produced each time he touched the poisoned animal, and this fact has led other physiologists to the capital theory that the reflex faculty of the spinal cord is very much increased by these substances. As a means of treatment Magendie proposed infiltration of the lungs.

In the paper in which Magendie explains his researches on substances containing strychnia, he proposes to make use of nux vomica as a medicine, when it is necessary to increase the power of action of the spinal cord. It is to him, to Fouquier, and to Andral, that physicians are indebted for this powerful remedy.

In his researches on the absorption of poisons, Magendie discovered an interesting fact, which had been forgotten, and has been revived a short time ago by Professor Bouley; it is that the stomach has very little power of absorption.

Few publications of Magendie excited so much public attention as his paper on vomiting. It was admitted that Haller, Kepler and some other experimenters, had well proved against Bayle, Chirac and Duverney, that the stomach is the most active agent of vomiting. M. Magendie proved positively that vomiting may take place without the stomach; but he went too far, in considering the diaphragm and the muscular walls of the abdomen as being the active parts in the act of vomiting. He, nevertheless, did not say, as he is accused of having said, that the stomach never has any active share whatever in the act of vomiting. In this paper, and in another very remarkable one, on the action of emetics, he relates experiments, proving that certain substances may produce vomiting, after having been injected into the veins. This fact had been known and forgotten—long before Magendie, Professor Wren, of Oxford, in 1666, had discovered it, and Fabricius, Schmidt, and Schleger, had confirmed Professor Wren's discovery.

In a paper on the epiglottis, Magendie established that this fibrous apparatus may be taken away, without any decided effect on deglutition; and that, during this action, the vocal cords of the larynx become approximated, so that the food cannot pass through the glottis.

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I shall not say much of a long paper of Magendie's, on prussic acid. I wish merely to recall the attention of practitioners to a plan, perhaps too much neglected—it is the employment of prussic acid in coughing, particularly in consumption. According to many instances reported by Magendie, this remedy appears to be, when carefully employed, a very useful one.

A very interesting paper of Magendie's, on non-nitrogenized food, attracted much notice some forty years ago. The principal object of this paper is to show that this kind of food is not sufficient to maintain life. Another great principle of dietetics proved by him in this paper is, the necessity of a variety in food. He explained the results of his experiments, showing that the cornea of animals insufficiently fed becomes ulcerated.

In the year 1818, Magendie published his paper on gravel, which is a model of clearness, conciseness, and good reasoning. One of his experiments, in which he had the assistance of Chevreul, led him to some important deductions as to the proper regimen for persons having the uric gravel. He found that a mammal, fed only upon sugar, gum and oil, had no uric acid in his urine.

In a paper, on the introduction of air into the veins, which was published long before the celebrated researches of Amusat, Magendie pointed out the danger of operations upon the neck. He proposed a mode of treatment, which may be successful when employed almost immediately after the introduction of air into the veins. He proposed to introduce a catheter into the right auricle, into the wounded vein, and to draw out, by a pump adapted to the catheter, the air and the blood contained in the heart.

Much has been said of the importance of Magendie's researches on the physiology of the roots of the spinal nerves. It may be admitted, that the first clear idea of a physiological distinction between the anterior and posterior roots of the spinal nerves belongs to Alexander Walker, and also that the demonstration of the great principle, that there are differences of functions in different nerves, belongs to Sir Charles Bell; but it was Magendie who proved positively the differences of functions between the anterior and posterior roots. The experiments and pathological cases published by Sir Charles Bell, had clearly established the differences between the seventh and fifth pair of nerves; but his experiments on the roots of the spinal nerves were not at all decisive. To Magendie belongs the merit of having proved this difference. There were two methods to be employed to

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ascertain this distinction, and Magendie made use of both. One of them was to divide either all the posterior, or all the anterior roots of one of the limbs of a living animal, and then to examine the effects of the section. This Magendie did, and he found that, after the section of the anterior roots, movements are entirely lost, and sensibility remains; and that after the section of the posterior roots, sensibility is completely lost, and movements are a little diminished. Another method of research is to employ excitation on the roots. His method at first led Magendie to erroneous views, and it will be shown hereafter that it was very difficult to avoid being mistaken. Magendie made use also of another method, which is somewhat of a compound of the two preceding. He divided all the posterior roots of a limb of a living animal, and poisoned it with nux vomica. Convulsions ensued in that limb as well as in the others. He then divided all the anterior coats of the limb of another animal, and gave it nux vomica. Convulsions took place everywhere, with the exception of the limb operated upon. This was certainly a decisive experiment. Two facts which have been discovered after these experiments of Magendie, explain why he obtained some results which led him to the erroneous views above mentioned.

One of the facts before mentioned is what Magendie called recurrent sensibility, and the other is, that the posterior roots may produce local reflex movements.

Magendie discovered the recurrent sensibility in 1839, and since that time Kronenburg, Volkmann, Claude, Bernard, and Dr. Brown-Séquard, have acknowledged the exactness of his experiments. When the anterior roots of one of the spinal nerves is pinched or burned in a living animal, in which the posterior roots of the same nerves are entire, pain is felt. Now, if the anterior roots are divided, and if the centric and peripheric parts of them are alternately pinched or burned, the centric part (the one in direct connection with the nervous centres) is found completely deprived of sensibility, and on the contrary the peripheric part (which seems to have no more connection with the sensorium) gives pain, so that this part appears to be sensitive, and it is this sensibility which Magendie called recurrent sensibility. The channel by which pain is transmitted to the sensorium, when the anterior roots are irritated, is in the corresponding posterior roots. This is well proved by an experiment of Magendie, consisting in the division of the posterior roots of a nerve, after which operation the anterior roots of the same nerve may be irritated without giving pain.

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From experiments by Dr. Brown-Séquard on this subject he has arrived at this result, that there is no sensibility at all in the anterior roots; but, that when they are strongly irritated, they produce a cramp in the parts of the muscles where their fibres are distributed, and, as there is a cause of irritation of the sensitive nerve fibres in the muscles, which have cramps or any violent contraction, pain is felt by the animal; therefore, it is because the anterior roots are motor, and able to produce a violent contraction in muscles, that they give pain when they are irritated.

In searching whether the posterior roots of the spinal nerves are only sensitive, or if they are sensitive and motor, Magendie frequently found that a local movement took place when he irritated these roots. Not knowing reflex action, he wrongly concluded that these sensitive roots were partly motor; this cause of error, and that arising from the existence of the recurrent sensibility, led him to be in contradiction with himself as regards his conclusions. As to the facts which he has related, although in appearance contradictory, they are all perfectly correct; and now that our knowledge is more advanced than when Magendie performed some of them, we may understand that the supposed contradiction is more apparent than real.

As regards the columns of the spinal cord, Magendie did not succeed in making out their functions, and we must acknowledge that this part of physiology, although of late much pursued, is still very obscure. Nevertheless, Magendie proved, first, that the posterior columns of the spinal cord appeared to be very sensitive; 2nd, that the anterior columns seemed to be much less so; 3rd, that the central grey matter is deprived of sensibility.

The researches made by Magendie on the physiology of the cerebellum, the corpora striata, and other parts of the encephalon, have not been so successful as his previous researches. His conclusions are not sufficiently well grounded; but we are indebted to him for many interesting facts, and particularly as regards those strange rotatory movements which are produced by alterations of some points of the encephalon. Magendie and Professor Flourens share the honor of having called the attention of physiologists to these mysterious phenomena.

Perhaps the most important discovery of Magendie is that relative to the influence of the section of the trigeminal nerve upon the face, and upon the organs of the senses; the alterations taking place in the eye are particularly interesting. But Magendie went too far in thinking that the trige-

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minal nerve might supply the place of the nerves of the superior senses. Among the latest discoveries made by Magendie, may be pointed out the fact, that sugar is found in a notable quantity in the blood of animals whose food contains much starch, and that the saliva of the parotid gland is quite different from the saliva of the submaxillary gland.

His researches on saliva were made in conjunction with Rayer and Payen.

In the year 1823 a strange attempt to save the life of a man attacked with hydrophobia, was made by Magendie. He injected nearly two pounds of water into the veins of this man, who was then almost dying. The symptoms of hydrophobia disappeared, but the patient died nevertheless, eight days after the injection of the water, without any more symptoms of hydrophobia. Whether death was caused by the injection of the water, or by a disease of the knee-joint, with which the man had been for some time affected, we cannot tell. Although this case was not a favorable one for this mode of treatment, and although Gaspard and others have not been successful when making use of it, yet numerous experiments might be performed on mad dogs, on a larger scale than have yet been made, to ascertain the positive value of the injection of water.

The publications of Magendie consist of two parts; those contained in papers, and those in books. Mention has been made of the discoveries explained in his papers, as they are scarce and somewhat forgotten. As to his books, since they are to be easily found, it will be sufficient to give a list of them. The two most important, are his "Précis de Physiologie," 2 vols. 8vo. and his "Leçons sur les Phénomènes Physiques de la Vie," 4 vols. 8vo. The first of these works has had great success; it has been translated into many foreign languages, and has reached a fourth edition in France. The second work contains an *exposé* of the extensive researches of Magendie on the blood and on animal heat. The other books of Magendie are—1st. "Leçons sur les Fonctions et les Maladies du Système Nerveux," 2 vols. 8vo. 2nd. "Recherches sur le Liquide Céphalo-rachidien," 1 vol. 4to. with an atlas. 3rd. "Leçons sur le Choléra." 1832. 1 vol. 8vo. 4th. "Formulaire pour la préparation et l'emploie de plusieurs Médicamens nouveaux," 1 vol.

The services rendered by Magendie to science do not consist only in his discoveries and his publications; he gave also a great impulse to the progress of physiology by his example, by his assistance and encouragement to young physiolo-

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gists, and also by having contributed, with De la Place, to the foundation, by the Académie, of an annual prize for experimental researches in physiology.

Not only has Magendie rendered great service to science, but also to France and to humanity. There are many measures of public hygiene proposed by Magendie, which have been adopted by the government. After an important report of his, the laws regulating quarantine were rendered less troublesome to travelers and merchants. The poor in two of the Paris hospitals were fed mostly on gelatine soups; but a report of Magendie's proving that this mode of diet was quite unable to nourish, a better soup has been given to them.

Although somewhat stiff in his manners, Magendie was in reality very kind and amiable. He belonged to that rare species of *savans*, who think it their duty to assist young scientific men, and the truly scientific who needed his assistance always found him ready and willing to give it. He had no faith in theories; he believed only in well observed facts. His whole life was devoted to the establishment or discovery of facts. He was always ready to abandon any view, on any subject, if facts opposed to these views were clearly demonstrated to him by any one. He was always ready to re-examine any subject by experiments, if told that he had been mistaken about it. His nature was so singular in this respect, that it was sufficient to gain his esteem to prove to him that he had adopted a wrong view.

As a professor, Magendie was never popular. He followed no plan, and often at the commencement of a lecture did not know where his fancy might lead him. For persons whose object was to study the principles of science, his lectures were not at all adapted; but for those anxious to find how new discoveries were made, and wishing to become discoverers themselves, there could not be a better school than the course of lectures of Magendie.

Such was this great man, whose name will rank as high as any other experimenter in physiology. If we have to regret that he had not a faculty for generalization, we must not, nevertheless, blame him for not having tried to make generalizations. We ought not to examine whether a man has done what was best to be done; but whether if, with the faculties with which nature has endowed him, he has done the best he could. Judging Magendie by this rule, we have only eulogy to offer.—*Med. Times & Gaz.*

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CHRONICLE OF MEDICAL SCIENCE.

ANATOMY AND PHYSIOLOGY.

1. *Examination of the Blood of a Dog deprived of its Spleen.* By M. Vulpian.

From the reports of the *Société de Biologie*, for April 1855, we abridge the history of a dog, from which the spleen was removed in July 1848, and the description of the appearances observed after the death of the dog, in February 1855.

For six months preceding his death, the "spleenless" dog (*chien derate*) had suffered from a cutaneous affection characterized by falling of the hair, rapid desquamation, and the formation of crusts on different parts of the body. This disease prevailed, throughout 1854, among the dogs confined, for purposes of experiment, in M. Flourens' laboratory, where the spleenless dog was quartered. The intense cold of February increased the indisposition of the spleenless dog, and during the night of February 19th, he succumbed. This animal, before he fell sick with the skin disease, that is to say, for full six years, appeared to enjoy excellent health; he ate with appetite, and digested his food well.

No disorder of any of the functions was observed, except during the fortnight succeeding the extirpation of the spleen.* A few months antecedent to this operation, the spleen had been removed with success, from a bitch of the same size as the dog. The animals were placed together, and the bitch, having been lined by the dog, brought forth, at different times, several litters, which did not differ, either in the number of pups, or in the relative number of males and females, from the ordinary litters of dogs.

A few days before the death of the spleenless dog, M.

* A longitudinal incision was made below the costal cartilages. The spleen protruded with a few loops of intestine. The vessels and peritonæum going to the spleen were tied in mass; the pedicle was divided between the ligature and spleen, and the organ was removed. The intestines were reduced, and the wound united by interrupted suture.

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Vulpian examined the blood obtained from an incision in its ear, under the microscope. The blood contained white and red globules of normal appearance. The blood of an healthy dog which had undergone no operation was examined comparatively. The number of white globules seemed the same in both animals.

The necropsy of the spleenless dog was made the day of its death. The lungs were healthy; the thyroid bodies and the supra-renal capsules were of their ordinary size. The heart was large; the cavities were filled with demi-fibrinous concretions; the left ventricle was notably hypertrophied; there was a calcarious deposit on the aortic valves, with vegetations. The liver appeared perfectly healthy; its bulk was neither augmented nor diminished, and its minute structure was unaltered. The gall bladder was filled with fluid bile. There was no trace of the spleen. The splenic artery divided into two branches on the upper border of the pancreas. One branch was small, the other was almost as large as the original trunk. The latter went to the greater curvature of the stomach; it was the left gastro-epiploic. The former came to an abrupt termination, (whence five short vessels proceeded to the great cul-de-sac of the stomach,) and was evidently the splenic artery.

The blood of the principal vessels was carefully analyzed. The red corpuscles (seven hours after death) were already deformed, and dentated at the edges; the white globules were intact. The white globules were everywhere numerous; perhaps they abounded in the inferior cava and spermatic veins more than elsewhere.

During the six and a half years that he survived the removal of his spleen, this dog had *never* eaten anything but raw meat. It was interesting, in reference to the theories of glucogenesis, and the opinions of physiologists who hold that the destruction of the spleen has a great influence on the functions of the liver, to ascertain whether the liver contained sugar. The dog's liver was therefore treated by alcohol. The filtered liquid was evaporated to dryness; the residue was dissolved in water, which gave the clearest results with the potash and sulphate of copper test, and with potash alone; the liquid was then allowed to ferment, and carbonic acid was abundantly disengaged.

Kölliker and the rest must now frame new hypotheses for the functions of the spleen!

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2. *On the Secretion of Sugar and Bile in the Liver.* By Dr. J. MOLESCHOTT.

In the Comptes Rendus of the French Institute, (May 1855,) we find a communication from the eminent physiologist of Heidelberg, Professor Moleschott, designed to report a series of experiments which he published in 1852, but which are not generally known, which tend to prove that the liver is not an excretory organ.

Professor Moleschott has succeeded, in a large number of cases, after extirpating the livers of frogs, in keeping these animals alive for two or three weeks after the operation. It is well known that after the kidneys are removed, urea accumulates in the blood. Now, if the liver is only an apparatus of filtration, we should expect to find organic acids, coloring matter of bile, and sugar, in the blood or tissues of animals from which this viscus has been removed. Dr. Moleschott examined the blood, muscles, gastric juice, and urine of frogs from which the liver had been removed from one to twenty-one days, repeating and varying his analyses. He could not detect a trace of sugar, or of the immediate elements of bile.

He concludes that sugar and bile are formed in the liver. For the first fact, science is indebted to M. Claude Bernard. In regard to bile, Müller first proved that it was produced by the liver. Kunde and Lehmann corroborated his experiments. But these savants preserved life in the animals in which they extirpated the liver for only two or three days.

Frogs from which the liver has been removed exhale far less carbonic acid than the same animals when intact. Dr. Moleschott has compared frogs without livers with frogs from which he had removed the legs, in order that they should lose as much blood as in excision of the liver, and also with intact animals. All the frogs were from the same marshes, of the same sex, as nearly as possible of the same size. The experiments were made the same day, in a room of equable temperature, and under the same atmospheric pressure. Twenty-six experiments were made for each category. One hundred grammes of unmutilated frogs exhaled in twenty-four hours 0.566 of carbonic acid; 100 grammes of frogs of which the legs had been amputated, gave 0.457; and the same weight of frogs without livers, gave 0.332. These figures give the average or mean result of the numerous experiments. These facts prove that the removal of the liver diminishes the elimination of carbonic acid in a degree altogether beyond what might be explained by the loss of blood inevitable in such an operation.

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MEDICAL PAT

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very active antiphlogistic treatment. There had, moreover, been diarrhœa for a week, and the ear perceived diffused sibilus in the left lung. Dr. Aran ordered six scarified cups the first day, and small doses of calomel, with mercurial frictions thrice daily, with a view to hasten ptyalism.

The treatment was not successful. Two large blisters were successively applied to the præcordial region, but the symptoms were not arrested. The difficulty of respiration and circulation increased steadily. In three days, the pulse became feeble, irregular, and extremely frequent. The mercurials were pushed, and at last the gums were slightly touched; but there was no amendment.

Finally, the symptoms became so menacing, that it was necessary to adopt some heroic means, or, it was evident, the patient would die. On the 7th of August, he passed a wretched night, being constantly threatened with suffocation. The dullness had extended an inch to the right of the sternum; the liver was displaced; neither the sounds nor the impulse of the heart could be perceived at the lower part of the chest; respiration 40, pulse 120.

Dr. Aran decided to puncture the pericardium. Of the three methods set down in French manuals: that of Riolan, trepanning the sternum—that of Larrey, puncturing in the space between the ensiform cartilage and the left false ribs—that of Senac, puncturing in the fourth or fifth intercostal space—Dr. Aran chose the last, which was successfully employed two years ago by M. Jobert (de Lamballe). He used a capillary trochar, as he had done in puncturing cysts of the liver, being satisfied that even if the walls of the heart should be unfortunately wounded, such an instrument would not produce an immediately fatal hæmorrhage.

He selected a point in the fifth intercostal space, where the dullness was complete, and the cardiac sounds altogether inaudible, and here removed a bit of skin. He then slowly introduced the trochar, directing it inwards and slightly downwards. The stylet was withdrawn once and no liquid followed; it was then reinserted, and pushed a little farther, when the cavity of the pericardium was reached, and the liquid escaped rapidly through the canula, flowing in jets.

The liquid evacuated measured 28 ounces (850 grammes.) The dullness diminished as it flowed; the pulse descended from 120 to 96; the cardiac sounds and impulse became distinct,

Dr. Aran, fortified by his experience in pleurisy, thought that he should not rest satisfied with a palliative puncture. He injected on the spot a mixture of water and tincture of iodine, each an ounce and a half, iodide of potassium, one scruple. The injection produced no pain. A portion of the liquid was allowed to escape; the wound was then closed by graduated compresses.

The dropsy returned quickly, and in a fortnight the symptoms were as bad as ever. On the 19th of August, Dr. Aran punctured again, and gave issue to 45 ounces (1,350 grammes) of fluid, which was albuminous and of the color of bile. It flowed in a jerking stream, as in the first operation. The same quantity of diluted tincture of iodine was injected as on the first occasion; the proportion of iodide of potassium was quadrupled. After this injection, some air found its way into the pericardium, and gave rise to very peculiar stethoscopic sounds.

The effusion commenced to return the evening after the operation, and accumulated until the 23d of August, when it gradually diminished. During September, symptoms appeared of pulmonary tuberculisation, and the lower extremities became œdematous. Gradually, however, these unfavorable symptoms yielded, under a treatment by blisters and vapor-baths. In the latter part of October, with the exception of his

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cough at night, the patient had nothing to trouble him. The physical signs of tubercular deposition of course remained, but in other respects the patient appeared like a well man.

Dr. Aran thinks that this case conclusively demonstrates the propriety of iodine injections into the pericardium. We do not propose to criticise it. We give it simply as a link in the history of an important therapeutical method. Velpeau did not hesitate to include the pericardium among the serous membranes susceptible of being treated by iodine injections, when he wrote his tract on the subject in 1846; and since the recent operations of Skoda, Behier, and Jobert have removed the dread of opening this sac, it was to be expected that the anticipations of the great master would be realized.

2. *On Injections of Borate of Soda in the Diarrhœa of Children.* By Dr. Bouchut.

The difficulties in checking the diarrhœas of children occurring during the period of the first dentition, are well known. Some physicians regard this flux as an enteritis; others consider the disease as originating in the large intestines. Dr. Bouchut, in the Annuaire de Medécine, 1855, reports at length his views on this subject, which we will condense with some care, and also suggests the topical treatment by injections of borax as having been found efficacious in the cure of these diarrhœas, which have often come under his charge at the hospital Sainte Eugénie.

The author divides the diarrhœa of children into two classes—the one symptomatic of lesions of the mucous membrane, and the other idiopathic, catarrhal, a true flux of the large intestines, causing death without leaving any appreciable cause to account for the fatal termination. The following cases best describe the disease and the method of cure adopted by Dr. Bouchut:

Case i.—A girl ten years old entered the Salle Ste Marguerite with a diarrhœa of five months standing, and accompanied with a quotidian fever. The next day an injection of two and a half drachms of borax was thrown into the intestines; on that day she had but one stool. Every day she used the injection, and a daily stool of a consistent nature was the result. She was discharged cured in six days.

Case ii.—A child, aged four years, entered on the 19th of July with a diarrhœa following upon typhoid fever. It had existed for more than a month, having eight or ten passages a day. After using the injections of borate of soda, she had but one stool in the twenty-four hours, of a semiconsistent dark appearance.

Case iii.—A little girl, three years old, entered the hospital on the

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19th of July with chronic bronchitis, evidently of a tuberculous nature, and very profuse diarrhœa. The borax checked the diarrhœa, and her general health was much improved.

CASE IV.—A girl of four years of age in the Salle Ste Rosalie, had impetigo of the head and a very profuse diarrhœa with twenty or thirty discharges a day. An injection of ten drachms of borax at once arrested the diarrhœa, and it never reappeared.

The result of these and many other similar facts has led Dr. Bouchut to regard this remedy as valuable in the treatment of the diarrhœal fluxes of children. The salt acts as an astringent and as an alkali, neutralizing the acid contents of the large intestines, which irritate the mucous membrane, and thus produce ulceration. We may suppose that the borax modifies the intestinal membrane, as we often behold it modify the mucous affections of the bucchal cavity in children. It has the advantages over the other injections used under like circumstances, and especially of the nitrate of silver, that it is not easily decomposed; that it is not irritating, and particularly that it exerts as an alkali a peculiar influence on the contents of the bowels.

The formula for this injection is ℞ *Borate of soda* ʒ *iii to* ʒ *x; eau sacrée* ℥ *viii.* As the borax is not very soluble, the quantity of water should be increased if the salt is administered in larger doses.

M. Bouchut has also used the borate of soda internally (by the mouth) in doses of thirty grains dissolved in gum water, but his observations are not sufficiently numerous to determine its value thus administered.

3. *A Glass Vial swallowed by Accident.* Reported by M. RAMOS BORGUELA.

The list of foreign bodies which have traversed the digestive tube is already very long, but we have never yet heard of a glass bottle having been swallowed; yet the observation of M. Ramos proves that this is not impossible. It is true that the bottle in question was a little flask about a finger and a half in diameter, which was unintentionally swallowed by a child thirteen years old, and was passed from the anus in about fifty hours.

M. Escolar, in the same journal, El Siglio Medico, reports an instance, not less curious, of a child five years old, who swallowed a glass pendant as long as the finger, which sojourned three days in the bowels, and made its appearance by way of the anus. In this instance the angles of the crystal were nearly effaced, and its polish destroyed by the action of the gastric juice.

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OPHTHALMIC SURGERY.

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1. *Iodine Injections in Hydropthalmia.* By Professor BONNET.

We find the following letter from the celebrated surgeon of Lyons to M. Velpeau in the Compte Rendu of the French Institute, Nov. 1855:

"Since the date at which you enriched therapeutics with the method of iodine injections in dropsical effusions in the serous cavities, the applications of the principles you laid down have multiplied in your hands and in the hands of your pupils; the limits apparently assigned by prudence have been transgressed with safety, and iodine has been injected into the pleura, the peritonæum, and the spinal arachnoid.

"While this generalization has been going on, one closed cavity has hitherto escaped your method; I refer to the eye distended by serum. I have thought that there was no propriety in exempting hydropthalmia from the treatment of other local dropsies, especially as the usual treatment of this affection is either insufficient, as in the treatment by puncture, or dangerous, as in partial excision.

"I have used iodine injection in two cases of hydropthalmia. The first was treated nearly three years ago with unfavorable results. A few months after the operation cancerous melanosis occurred, and made it necessary to extirpate the ball. I attributed the failure which had at first discouraged me, to the latent organic lesion which complicated the dropsy. The second case was simple. The result was identical with the usual result of the operation for hydrocele. An inflammation lasting a few days was followed by a gradual atrophy of the eye, which appeared at the end of a few months as a small opake globe sunken in the orbit.

"Although these cases are insufficient to establish the superiority of iodine injection over the other modes of treatment of hydropthalmia, the last one proves that this method can succeed in this affection as in other dropsies of closed cavities.

"The liquid which distended the eye in these cases was serous, and flowed readily through a fine trochar. It contained albumen, and coagulated by heat and acids, characters which distinguish it from disorganized vitreous tissue, which contains, according to Berzelius, only 2-1000 of albumen, and cannot be coagulated by heat.

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"These facts indicate that hydropthalmia is not an hypertrophy of the natural humors of the eye, as is generally admitted, but a serous secretion, requiring the same treatment as other local dropsies."

2. *Ectropium of the Upper Eye-lid—Keratitis. Treatment by Suture of the Eye-lids.* By PROFESSOR NELATON.

We have here described, in the clear and forcible language of the eminent professor of the Hôpital des Cliniques, a most aggravated case of ectropium of the upper lids of both eyes, accompanied by great and growing disorganizations of the eye proper, arising from the constant exposure of the ball to the action of light and its liability to the irritation of foreign bodies, &c. whilst the complete eversion of the lid rendered the unfortunate person hideous in the extreme. We transcribe the report of this case from notes taken by MM. Lorain and Cadet Gassicourt, and published in Jamains Annuaire de Chirurgie, 1855.

In this case of double ectropium the edge of the lid touched the eye-brows. The lid was so returned upon itself that the upper border of its tarsal cartilage had become the lower, and its internal face was now the external. The cornea was deformed and conical, covered with opacities and almost impervious to light. The conjunctival and sclerotic coats were injured, and the sight nearly lost.

M. Nelaton, after rapidly reviewing the various operations for blepharoplasty, proposed by Celsus, Wharton Jones, Dieffenbach, Fricke and others, determined to adopt the suggestion of Maissonneuve, and proceeded to attempt the cure of the ectropium of the left eye-lid by sowing together the upper and lower lids, with the hope of producing permanent union, and by this method keep down the everted cartilage until time had remedied the evil, when the connection with the lower lid might be easily severed. The operation was thus performed:

Having first made a free transverse incision of the upper lid just below the eyebrows, M. Nelaton proceeded to shave off the edges of both lids at three different points and then carefully brought together the freshened edges by means of three strong sutures. Cold water dressings were applied, and the patient was put to bed.

Finding on the next day that the upper lid had not assumed a normal position, an *epingle d'insecte* was passed first through the lower and then through the upper lid, so as to approximate them to each other, and this was permitted to remain.

Seven days afterwards the pin was withdrawn and the

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ligatures cut. The eye lids were united together firmly at three points, and the upper lid had assumed nearly a normal aspect. The discharges from the eye passed freely between the points of union. Sixteen days after the operation, M. Nelaton introduced the boy to the class. The ectropium was apparently cured; the transverse incision had healed, and the only question with the surgeon was, when it would be safe to sever the connections between the two lids and permit the use of the ball again. Maissonneuve's operation had failed, because he released the upper lid from its restraining connection with the lower lid too soon, when the ectropium quickly returned.

M. Nelaton therefore determined to wait for a considerable length of time before he permitted the two lids to be separated. Unfortunately, however, this young man, in a month afterwards, was attacked with typhoid fever and died. The positive success of this operation could therefore not be proved; but the surgeon, in making the post mortem, released the two lids, which retained their natural position, whilst a very remarkable fact occurred, showing the advantage which had resulted to the eye operated on over the other which had been permitted to remain intact. Under the influence of the fever the eye still exposed to the light became dry and hard, like the lips, tongue and nasal openings. Suppuration followed, and the eye was completely destroyed. The eye operated on retained its natural moisture to the last, and when the lids were parted after death, the cornea had recovered to a great degree its natural transparency, and the opacities had much reduced in size.

But for the intercurrent malady, M. Nelaton had designed to have gradually severed the adhesion between the two lids and there was every reason to believe that a perfect cure would have resulted.

OBSTETRICS.

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1. *On Pregnancy in Cases of Deformed Uterus.* By Dr. Chiari.

As our readers are aware, the uterus in the human subject is bifid, as in many quadrupeds, until the end of the third month, when the angle between the orifices of the Fallopian tubes begins to be effaced, and the uterine cavity assumes its

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triangular form. In some cases, however, the septum remains, and constitutes the faulty conformation variously described by authors as bifid, bilobate, bicorned, bipartite, bicornute, bilocular or double uterus, which has been the subject of a monograph by Canestrini, entitled *historia de utero duplici.* Another malformation consists in an absence of one horn of the uterus in the early stages of embryonic life, which results in an unilocular womb, having but one Fallopian angle, one ovary, etc.

Impregnation may occur in either case. Occasionally each cavity of the bilobate womb is occupied by a fœtus. An interesting case of this sort was inserted by Dr. Peebles in the third volume of the Virginia Medical and Surgical Journal, p. 157. A similar case is recorded in Rust's Magazine, volume xx, p. 568, and another in the Ephemerides of Montpelier, by Duges. The unilocular uterus is much less common, and instances of pregnancy in this condition are consequently rare. We find an instance of it, however, in Dr. Chiari's paper, of which the title is given at the head of this article. It is published in the Annali Universali di Medicina, of Milan, for 1854.

Case.—A pregnant woman, affected with syphilis, entered the Lying-in hospital of Milan, Nov. 4, 1853. She was delivered, without any unusual phenomenon, of a seven-months' child, which died on the fifth day of sclerema. The mother died on the eighth day of endometritis.

At the autopsy, they found an *unicornate womb*, of which the entire left side was wanting. The left horn, left oviduct, ovary and round ligament were all absent. The left fold of peritonæum passed from the lower part of the womb to the wall of the pelvis. The fundus uteri inclined to the right; its walls were of uniform thickness.

In the Revue Médicale, June 1831, Vidal reports a case of unilocular uterus, found in a dissecting room. Chaussier exhibited a similar specimen to the faculty of medicine. It was taken from a woman who died at the Maternité, after being delivered of her tenth child. A single tube and ovarium extended to the right side.

It was suggested by Dr. Pendleton, of New York, in one of the early numbers of the American Journal of Medical Sciences, that menstruation during pregnancy may depend on the existence of a bilobate uterus, and that the instances of superfœtation on record are only examples of successive impregnations in the two portions of wombs presenting this vice of conformation. These views were enforced by a great number of examples in a work published by Cassan,

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two years before Dr. Pendleton's essay. Cassan's work was entitled: *Recherches anat. et physiol. sur lescas d'uterus double et de superfœtation.*

2. *Detachment of Placenta by Injection of Funis.*

It is well to review occasionally the facts on which recommendations of unusual remedial means are based. With this purpose, we may glance at the testimony in favor of injecting the funis in cases of retained placenta, a proceeding referred to in connection with the case of Dr. George Minor, in the fifth volume of the Virginia Medical and Surgical Journal, p. 298.

This method was first suggested we believe by Dr. Mojon, a Genoese physician, in the Repertorio di Medic. of Turin, for May 1826. He employed cold water acidulated with vinegar, which was forcibly injected by the umbilical vein.

In the Annali Universali di Mèdicina for 1827, Dr. Tartoni, of Milan, relates a case in which he successfully adopted this practice.

Dr. Hopkins states, in the London Med. and Phys. Journal for 1827, that he has twice resorted to this plan with the best results. Two hours had elapsed, in each case, after the birth of the child; pressure, cold, and other means had been unsuccessfully employed. In three minutes after eight ounces of cold water had been injected by a common syringe, uterine contractions came on, and the after birth was forced into the vagina.

Dr. Jemina reports, in the Repertorio di Medicina, Aug. 1828, three cases of uterine hæmorrhage resulting from partial attachment of the placenta, in which its detachment was effected by cold acidulated injections through the umbilical vein.

Dr. Doucet, of New York, publishes a case, in which he adopted this treatment with success, in the N. Y. Med. and Phys. Journal, Dec. 1828.

Duparque records two cases in which this means was employed with success in the Nouvelle Bibliothèque Médicale for April 1828.

In Rust's Magazin for 1828, a case is related by Dr. Hoffman, in which there was *adherent* placenta and internal hæmorrhage. The injection of some *warm* water and spirits by the funis was followed in one minute by the expulsion of the placenta.

In July 1829, the Annali Universali, of Milan, contains an-

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other case, in which this plan was resorted to with success, by Dr. Chiésa.

Adding to these, cases reported by Lemaistre, Liegard and Sandras, we have fifteen cases in favor of this practice from highly respectable sources; and although it is not *proved* that the placenta was adherent in all these cases, such a body of testimony should certainly lead us to employ this simple remedy in cases of supposed adhesion with flooding, after using the ordinary resources without success.

TOXICOLOGY, &c.

1. *On Chronic Poisoning by Chloroform.* By Dr. Fock.

In a paper with this title, published in one of the numbers of the Deutsche Klinik for the latter part of 1854, Dr. Fock records the following remarkable case. It is translated in the Gazette Médicale, Nov. 3d, 1855, whence we copy it:

Case.—A man of thirty-four was admitted at Professor Langenbeck's Clinic, to be treated for anchylosis of the knee by the method of that surgeon. He was of scrofulous constitution; his complexion was of a purplish red, and his face indicated a disposition to cerebral congestion.

Two drachms of chloroform were poured on a compress which was held to the patient's mouth. In a few moments, he was narcotised.

It was necessary to remove the compress before the dressing was finished, for the face had become livid, and the respiration stertorous. He was readily revived; breathed freely, and complained of pain. An hour and a half afterwards, he was observed to be sleeping soundly. Being aroused he said that he felt well, that he had some pain in the knee, that he was thirsty; after drinking, he immediately fell asleep. He was aroused from time to time by sprinkling of cold water and by ammonia held to his nostrils. Towards night, his condition seemed alarming; his head was hot, face livid, pulse small and irregular, heart's impulse imperceptible, jugular veins turgid, respiration laborious, eye-balls turned upward, pupils dilated; he did not reply to questions; his breath had no odor of chloroform. He was largely bled. The blood was black and thick; it coagulated slowly; no odor of chloroform could be perceived in it.

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As the coma did not diminish, the patient was placed in a bath, and cold water was poured on the head and chest. This produced a slight amelioration, followed presently by a return of insensibility. An enema was given of vinegar and water, sinapisms were applied to different parts of the body, and the surface was irritated with a flesh-brush.

After three hours of continual persevering exertion, the respiration and pulse became better, and the patient took a few doses of ether. Yet the drowsiness persisted, lasting four days altogether. Twenty leeches were applied behind the ears, and the patient had a dose of calomel and jalap. The functions were slowly re-established, and the patient resumed his ordinary habits.

This case clearly shows a rapid and powerful impression of chloroform on the nervous system; it is evident that, except for the prudence with which the anæsthetic was administered, and the energetic and assiduous after treatment, this patient would inevitably have succumbed.

2. *Deaths from Chloroform.*

Since we last referred to the subject, a number of cases have been reported in which death resulted from chloroform inhalation. We shall mention enough of the details of each case to identify it:

Case I.—A middle aged man entered University college hospital, with retention of urine. Mr. Erichsen having failed to pass the catheter, resolved to make a second attempt with the aid of chloroform, being prepared to puncture by the rectum in case he was unsuccessful. A small quantity of chloroform vapor was inhaled, but sensibility remaining, an additional quantity was given. The face became congested, the respiration slow and laborious. The tongue was drawn forward, and water was dashed on the face and chest of the patient. Artificial respiration, galvanism, etc. were diligently employed, but without any result. The patient was apparently dead in three minutes from the commencement of the second inhalation. We have no account of the necropsy.

Case II.—A woman of 56 years was about to undergo amputation of the leg at Guy's hospital. A drachm of chloroform had been inhaled, a second drachm had been poured on the lint from which the patient was inhaling, and Mr. Birkett was about to commence his incisions, when the assistant at the femoral artery exclaimed that this vessel no longer pulsated. At this instant, the patient heaved a long

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sigh, and then respiration ceased. The means adopted in the first case were employed in this with no better success. At the autopsy, the lungs were found congested; the left side of the heart was empty, the right side was engorged; the muscular fibres had undergone fatty transformation to a great extent. The blood was everywhere fluid and dark. Both of these cases are reported in the Medical Times and Gazette.

Case III.—We learn from the Boston newspapers of January 9th, that Ida Morgan, a prostitute, recently died in that city in consequence of inhaling chloroform for the purpose of having a tooth extracted. The dentist who administered the anæsthetic states that he used "the usual quantity." The patient had previously experienced trouble in taking chloroform. Three attempts to produce anæsthesia were made. At the third application the patient attempted to rise, and then her head rolled heavily to one side. She was moved to a couch, and "her breathing became easy," but she never spoke afterwards. Drs. Jackson and Stedman made an examination, which enabled them to testify to the absence of organic disease, general fluidity of the blood, and the purity of the chloroform. The coroner's jury felt called upon to warn the public against the use of this agent. No blame is attached to the dentist, Mr. Emery, of Broomfield street. It is not stated what restorative means were used.

Case IV.—One of the most recent instances of death from chloroform is found reported in the December number of the Edinburgh Medical Journal, and there is an additional interest attached to this case, in as much as Dr. Simpson, who may be regarded as the originator of this peculiar anæsthetic agent, was present in less than five minutes after the syncope occurred, and with his usual perseverance resorted to the remedies proposed under such circumstances; and after one hour and a quarter of the most energetic exertions, the case was reluctantly abandoned as hopeless.

Chloroform in this instance was administered to Mrs. H. a lady 36 years of age, for the purpose of extracting four teeth, by Dr. Roberts, a surgeon dentist of Edinburgh, who had administered the same agent to the same person on *four* previous occasions. The lady had also resorted to its use during a recent accouchement. Mrs. H. as she sat down in the operating chair, her husband being present, asked if Dr. R. had heard of that case of death from chloroform which had occurred lately in the neighborhood; and afterwards she said she felt "rather nervous." After inhaling less than one and a half drachms, she said, "you must not operate until I

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am insensible, I am not over yet," and *while speaking*, gave a convulsive start, and sank to the floor.

Drs. Simpson and Priestly at once resorted to artificial respiration, and galvanic currents were rapidly passed through the respiratory muscles. General bleeding was also employed, but not more than six ounces of blood could be obtained. A post mortem revealed nothing to account for the unfortunate result, although the *heart* was found preternaturally small, and some appearances analogous to a fatty degeneration of that organ.

3. *Docimasia Pulmonum.* By Dr. Blot.

In the report of the proceedings of the *Société de Biologie* for August 1855, we find a communication which attempts to prove that there are cases in which none of the tests employed by medical jurists will distinguish the lungs of the still-born from the lungs of the child born alive. The author reports the following fact:

A child, apparently born dead, was revived by the usual means, and after twenty minutes of assiduous effort, the respiration became regular; the inspiration was short and incomplete; the expiration was noisy. At the end of an hour the breathing ceased; the heart continued to pulsate for a short time, and then the child expired.

At the autopsy, the lungs occupied the back part of the thorax, filling barely two-thirds of the chest; the thymus and pericardium were exposed; the lungs were brown, felt like liver, and did not crepitate under the finger. *They sank at once to the bottom of a vase of water, and when cut in fragments each of the fragments sank.*

If a fragment was pressed under water, *extremely* fine bubbles of air were disengaged, though the finger felt no crepitus.

In conclusion, Dr. Blot insists on the importance of facts of this sort, proving that docimasia or testing of the lungs cannot always determine whether a child has lived or not, an opinion which coincides we believe, with that of most American and English medical jurists.

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THERAPEUTICAL RECORD.

Cysticercus in the eye.—The Deutsche Klinic publishes an operation performed by Dr. Von Græfe for the extraction of a cysticercus cellulosæ from the anterior chamber of the eye. The animal was taken alive and lived ten minutes in a little water. Its body was round, transparent, and about four millimetres in diameter; the neck six millimetres in length, and was surrounded by a deep blue ring. The eye had perfectly recovered on the sixteenth day.

Dysentery.—In a memoir on the *Diseases of Egypt*, published in Vierordt's Archiv. Professor Griesinger devotes a long section to dysentery, a disease so common in Egypt as to have furnished more than one-half of the autopsies made by him, (186 out of 263.) In 96 of these cases, the dysentery was primary; in 90, secondary. Dr. Griesinger treats successively of the morbid anatomy, etiology, symptoms and treatment of dysentery. We cite his article for the purpose of recording his notice of a remedy which was found extremely useful. This was albumen, in the form of white of egg. It was given by the stomach and in enemata. The whites of twenty eggs mingled with a quart of water were usually administered in the twenty-four hours, and the whites of ten eggs were given in injection. The pain and tenesmus often subsided after the first few doses; the stools became bilious and consistent; in a word, many grave cases were cured by the more or less protracted use of this remedy.

Gonorrhœa.—We copy from the Union Medicale of Aug. 28th, the following formula of an electuary employed for many years by M. Beyran, in the treatment of gonorrhœa: Take of copaiva ℥ iss; calcined magnesia ʒ i; alum (levigated) gr. xv; catechu (levigated) ʒ iss; cubebs ʒ ix; opium gr. xv; essence peppermint, do. canella, aa. gtt. xl. M. Make an electuary.

A teaspoonful thrice a day: most agreeably taken, enveloped in wafers made of unleavened bread, *pain azime*, which are much used in France for such purposes.

Icterus.—Dr. Gieseler of Göttingen calls attention of practitioners (Henle and Pfeuffer's Zeitschrift) to the utility of white of egg in certain varieties of jaundice. M. Bernard's

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experiments indicating that albumen was rendered assimilable only through the function of the liver, Dr. Gieseler suspected that this substance might arouse the action of that viscus, and promote the secretion of bile. He reminds us that Charles White is reported to have cured inveterate cases of jaundice by this remedy, which he used without any elaborate theory to commend it.

Iodide of quinine.—M. Pauva, a Neapolitan chemist, proposes the iodide of quinine in those cases of intermittent fever complicated with scrofulous affection. Dr. G. Mafredonia of Naples has made use of the remedy in such cases with marked success. He uses it in doses from four to eight grains daily.—*N. O. Hosp. Gaz.*

Luxation of the knee.—M. Royer communicated to the Imperial Academy of Medicine, 18th September 1855, a case of complete luxation of the knee forward, the tibia mounting many centimetres before and above the articulating surface of the femur, without tearing of the external parts. This luxation was easily reduced by forced flexion of the leg on the thigh, and a prompt cure was effected. In a month the patient could walk, and in six weeks the cure was completed.—*Revue de Therap. Med. Chirurg. Oct.* 15, 1855.—*Med. News, &c.*

Mammary abscess.—"We know," says Dr. Ratzenbeck, (Journal des Conn. Med.) "that cracks in the breasts are frequently the determinate cause of engorgements of the gland. The mucous lining of the galactophorous ducts participate in the exterior inflammation of the nipple. Exudations close up the ducts and prevent the milk from flowing, and inflammation is the result." Dr. Ratzenbeck advises that we should press the mamma with both hands gently towards the nipple, and a little transparent white vesicle appears, produced by the milk forcing itself through the closed ducts. Pierce the vesicle with a needle and remove the scaly epithelium which obstructs the tube. Apply the infant, and great relief will at once follow.

Morning sickness.—Dr. Kuechenmeister (Journal des Conn. Med.) contends that the distressing morning sickness which occurs in the early months of pregnancy results from emptiness. He proposes that all breeding women should take their breakfast in bed, and not rise for two hours afterwards. As the stage which precedes the vomiting bears some ana-

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logy to syncope, the repose in bed until after the morning meal is not an irrational suggestion.

Pneumonia.—A Hungarian physician, Dr. Stohandl, reports three cases of pneumonia, in which benefit was derived from the inhalation of small quantities of chloroform (30 to 40 drops) repeated several times a day. After each inhalation the symptoms were relieved; after four or six hours they again became aggravated, but were again relieved by a repetition of the inhalation. In from five or eight days a cure was effected.—*Revue de Therap. Med. Chirurg. Oct.* 1, 1855, *from Ungar Zeitschrift.*

Punctured womb of pericardium—recovery.—A case occurred to Mr. Erichsen, in which a man, stabbed with a knife in the fourth intercostal space, over the heart, presented the symptoms of wound of this organ, viz:—face pale and anxious, pulse 112, respirations 28 and painful, &c. He was actively treated by local depletion, calomel and rest, with success.—*N. Y. Journal of Medicine and Collateral Sciences.*

Racahout des arabes.—From a recent edition of Bouchardat's Formulaire, we take the following recipe for the racahout powder, which is now much used as an agreeable and nourishing diet with dyspeptics and convalescents. ℞ Dried cocoa ʒ iv; potato flour and rice flour each ʒ x; sugar ʒ xv; vanilla ʒ ss. One or more spoonfuls in ℥ viii of boiling milk.

The Wakaha des Indes is an analogous preparation, by some preferred to the racahout. We give the formula. ℞ Powdered sugar ℥ x; dried cocoa ℥ iv; vanilla ʒ i; canella ʒ iv; ambergris grs. v. Used in the same doses as the former compound.

Spermatorrhœa.—One of the editors of this journal has recently been using the bromide of potassium in spermatorrhœa, with happy effects. He exhibits it in doses of four grains, in solution, three or four times in twenty-four hours. It was conjoined with the use of cold water injections into the rectum, so as to empty it. A light vegetable diet was also directed. The result, in all cases of its use, was satisfactory.—*Cincinnati Med. Observer.*

Superfœtation.—Dr. Gilman, in the absence of a pathological specimen, desired to relate a rare fact, viz: A well authenticated case of superfœtation, occurring in a woman

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who was married on the 9th of October last. In nine months and one day, on the 10th of July, she gave birth to a male child, of large size; and last week gave birth to a female child, fully developed—the interval being seventy-two or seventy-three days. She was unable to nurse the first child fully, until the birth of the second, the supply of milk being deficient. He had not seen the patient himself, but intended to do so, and ascertain if she had a double uterus.—*N. Y. Acad. Med.*

Tenesmus in dysentery.—Dr. Ehrenberg highly recommends the administration of *vapor-clysters of chloroform* in tenesmus. Mode of administration: Take a common four ounce phial, in which a drachm of chloroform is poured. Adapt a common, but tightly closing tube, of india rubber or gutta percha, to the end of the phial. Provide the other end of the tube with a canula, similar to that of a common syringe, having several small openings, and introduce it into the rectum. To hasten the evaporation of the chloroform, the phial may be held with the hands, or warmed by immersing it into a vessel filled with warm water.—*Medicinische Neuigkeiten from N. J. Med. Reporter.*

BIBLIOGRAPHICAL RECORD.

I. *Obstetric Memoir and Contributions of* Professor Simpson of Edinburgh. Edited by W. O. Priestly, M. D. of Edinburgh, and Horatio R. Storer, M. D. of Boston, U. S. Vol. I. Philadelphia. Published by J. B. Lippincott & Co. 1855. (Received of J. Woodhouse & Co.)

The author of these essays has won for himself an extended and well merited reputation. The professor of midwifery in the ancient school of Edinburgh, Dr. Simpson, has been ever active in the special department to which he has devoted his life, and although not yet forty-five years of age, he has by his many valuable contributions to the current literature of medicine, his daring innovations on some of the heretofore established opinions of the profession, and especially by

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his original and striking suggestions and experiments, attracted an amount of attention hardly surpassed by any of his contemporaries.

We cannot hesitate to thank the editors of the work before us for their labors in collecting the many fugitive memoirs and essays of Dr. Simpson, which are the embodiment of his peculiar views and extended experience, and placing them before us in an orderly and attractive form. Many of these essays would probably never have come under the observation of the American profession, whilst there is hardly one of them which does not contain valuable and instructive matter.

Dr. Simpson must certainly regard himself as fortunate in obtaining the services of the gentlemen who have edited this book. At much cost of time and labor, they have brought together his contributions to the medical journals of Great Britain, many of which were already out of print, and arranged them in methodical order. They have transformed him from a fugitive essayist into a bulky and dignified author; they espouse all of his views with unlimited reverence, and defend them against all comers. The two greatest countries of the world are represented on his title page, and Boston and Edinburgh vie with each other in sounding his praises. Dr. Priestly, more moderate in his defense of his fellow townsman, avoids touching upon the personal differences which have arisen between the professor and other eminent members of the British profession, but Dr. Storer, with the characteristic impetuosity of his country, dashes into the thickest of the fight, and lustily flourishes his editorial rod over the heads of Dr. Lee of London, the French academy of medicine, and all others who have been disposed to raise an issue with his revered "master." We do not admire the good taste displayed by our ardent countryman, but are willing to give our readers, in his own language, the justification which is offered by Dr. Storer for his rather unnecessary warmth. He thus closes his prefatory remarks:

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In conclusion, we omitted these matters, as I have said, in the Edinburgh edition, and I have referred to them here, because in the one case they seem needed. In doing so, I have perhaps overstepped my bounds, and exchanged the coldness of an editor for the warmth—I trust still impartial—of a friend. If such is the case, I shall look for excuse to the peculiar relations in which I have stood to Dr. Simpson. For one year I was admitted to close intimacy with him—to his home. I learned to love him as my father; he treated me as a son. I am proud to call him master. In selecting an American as one of his editors, he testified to his regard for our country. I could only promise him a hearty welcome among us, and I am sure he will everywhere receive it.

It is impossible to give the contents of this work such an analysis, in our limited space, as they fully merit. The great importance of the subjects discussed and the many novel and daring innovations proposed by the author, in the most interesting branch of medical science, will fully justify us in recommending it to the profession. To give some idea of the field in which Dr. Simpson has indefatigably labored, we will append the following list of the original suggestions made by him to the department of obstetrics in the last ten years, which we publish in the order laid down by the American editor:

In 1844, Dr. Simpson proposed, in certain cases of placenta prævia, the extraction of the placenta before the child.

In 1845, the use of alkaline salts in placental phthisis.

In 1847, the operation of turning as an alternative for craniotomy.

And, in the same year, the use of anæsthetics in labor.

In 1843, Dr. Simpson proposed the sound in uterine diagnosis.

And, in the same year, the treatment of displacements by intra-uterine pessaries.

In 1844, the incision of the cervix uteri in obstructive dysmenorrhœa.

And in 1849, the employment of sponge tents to expose intra-uterine polypi.

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Regarding each of these latter proposals, many fears are still held, often openly expressed—though upon two of them, the first and the last, the sense of the profession is doubtlessly now with Dr. Simpson. Without the sound, no man can satisfactorily diagnose some of the diseases of women; without the tent, though the sponge is very likely yet to give place to some safer agent, there are others that no man can cure.

Whilst some of these views have not yet received the seal of general approbation at the hands of the profession, yet the man who proposed the use of chloroform, the separation and delivery of the placenta in placenta prævia; who invented the intra-uterine pessary, and justified the propriety of the operation of vaginal hysterotomy, deserves to be received by all with the respect which is due alike to the novelty of his innovations and the vital questions which they involve. We doubt not that the work in question will be eagerly sought in this country, and we know that its perusal will impart to many an amount of information which it would be difficult to obtain in any other way.

II. *Transactions of the American Medical Association.* Instituted 1847. Vol. VIII. Philadelphia. 1855. (Received from the Committee of Publication.)

It has been common of late to charge the American medical association with dereliction of duty. Having failed in its great object of reforming the profession and the schools, and having yielded to the enticements and good dinners of the professors, its enemies say that it no longer deserves the applause or even the support of the great commonalty of medicine, and must soon sink into perfect and well merited contempt.

The *eighth* volume of the Transactions of the Association contradicts in the strongest and most effective manner possible, this sweeping denunciation. It tells us of the last

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meeting in Philadelphia in 1855; it reads the roll of *five hundred and twenty-eight members*, representing *twenty-six* states of the Union. As we glance over the long list, we recognize the leading and active workers in the field of medical science—the old men have broken into the routine of long years, and have come from a thousand miles to do their duty and aid the cause, whilst the young and ambitious aspirant for future fame is present to learn wisdom from the grey heads around him, or to propose and defend some conception or theory of his own.

To make our defense more complete, we would point to the scientific contributions elicited by the late meeting, to refute the arguments of its enemies. They form a volume of seven hundred pages, and contain essays of undisputed value. Amongst them, we would especially notice a very elaborate report on the diseases of Missouri and Lowa, from the pen of Dr. Reyburn of St. Louis, which embodies many facts of great value. Shall we add, that although a committee appointed to prepare a like paper for Virginia and North Carolina has been in existence for *seven years*, we have never yet heard their report?

Another valuable contribution is by Dr. Hunt of Buffalo, upon the hygrometrical conditions of the atmosphere in different localities—a subject which, in its relations to the laws of disease, is worthy of the most profound study. Hamilton's paper on deformities after fractures, is worthy of attention; and the essays of Drs. Hooker and Davis on dietetics should not be passed over.

But the interesting feature of this volume of the Transactions is undoubtedly the prize essay on placenta prævia, presented by Dr. J. D. Trask of White Plains, New York. Whilst the successful essays of previous years may have been marked by more originality and boldness of innovation, this paper has added to the well deserved reputation of its author as a laborious and accurate statistician, and is almost exhaustive of the subject which it considers. We do not now propose to notice in detail the valuable deductions

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drawn from this immense mass of information with regard to placental presentations. We design to attempt this on a future occasion, but we again repeat, that if the Philadelphia meeting had resulted in nothing but the publication of this essay, its time would not have been wasted.

As we have alluded to the signs of dissatisfaction coming from certain quarters, in connection with the association, it gives us pleasure to state that in Great Britain and on the continent its deliberations have been greeted with decided approval. Its series of publications are considered of great value, and the association is regarded as one alike creditable to the profession and the country.

III. *On Bandaging and other Operations of Minor Surgery.* By T. W. Sargent, M. D. New edition, revised and enlarged. Philadelphia. 1856. Blanchard & Lea. (Received from George M. West.)

The title of this work states clearly its object. Not aiming at a description and history of the grand deings of the surgeon, it confines itself generally to the details and duties of the routine practice; and having reached a new and enlarged edition, it has a right to regard itself as an established favorite.

We think this book has been of use to the profession at large, and are therefore glad to bring it to our reader's notice. Everybody cannot be a Mott or a Warren. Napoleons are as scarce in the operating room as on the battle field. But every man in our country *can* become master of the daily duties and difficulties of surgery; and as we are practitioners of all the departments of the science, we should be prepared to meet the incidental cases which spring up about us, with correct views and a sufficient amount of dexterity. A study of this book will aid the general practitioner in keeping up with the progress of the art, for its author has, with commendable industry, introduced most of the modifications and improvements in surgery; and we are glad to

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see that the many additions made by the surgeons of our own country argue that they are not standing still on these points, but are striving to do their duty and are willing to add their proportion to the common fund.

IV. *A Dictionary of the Terms used in Medicine and the Collateral Sciences.* By RICHARD D. HOBLYN, A. M. Revised, with numerous additions. By ISAAC HAYS, M. D. Philadelphia. 1855. Blanchard & Lea. (Received of A. Morris.)

More modern than Hooper, more convenient than Dunglison, this dictionary is destined to meet with a favorable reception. In this matter, "the last is always the best," for the coinage of new words is going on so rapidly in our science, that unless we have a last edition of a dictionary, we are lost before the first page of our book is concluded. An antiquated professional brother, who visited us some time ago, remarked, as he turned over a bound volume of this journal, "that he was like Rip Van Winkle, when he waked up from his long nap, for he could hardly recognize the old landmarks and bearings with which he was once so familiar, and he was lost midst the newfangled technicals of modern days." We advised him to buy Hoblyn's Dictionary.

The name of Dr. Hays, on the title page of the book in question, assures us of the accuracy and completeness of the American edition. His own additions are not the least valuable portions of the contents. He has adapted it to the American student, by making its prescriptions conform to the United States Pharmacopœa, and in other respects enlarged and improved it, while, with the accustomed neatness of the publishers, the whole is rendered as convenient as possible.

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EDITORIAL AND MISCELLANEOUS.

The proprietors of this journal announce with much pleasure, that they have permanently engaged the valuable services of Dr. George A. Otis, who will hereafter, as associate editor, labor to increase the usefulness and originality of the work. The readers of the Virginia Medical and Surgical Journal especially, who have been long familiar with the accurate knowledge and scientific attainments of this gentleman, will unite with us in the feeling of satisfaction with which we make this announcement.

Mortuary Statistics.

The National Medical Association, at one of its earliest sessions, memorialized the different state legislatures for the passage of acts adopting a system of registration of marriages, births and deaths. This authoritative recommendation resulted in the passage of laws in several states, which seemed adapted to secure the object sought. It was at first unheeded by the general assembly of our state, but by the strenuous exertions of a few physicians, among whom it is right to make honorable mention of the present professor of physiology to the Medical College of Virginia, the matter was forced on the attention of public men. The Stethoscope, and the Medical and Surgical Journal repeatedly urged the profession to insist on the enactment of an efficient law on this subject. At last, by personal solicitation, our late lamented friend, Dr. Gooch, procured a favorable vote for the registration bill, which was passed during the closing hours of the last legislature. There was no time to perfect the minor details of the bill, which would have insured its operation. The duty of collecting the statistics was assigned

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to the commissioners of the revenue; but no provision was made for the remuneration of these officers, or for the purchase of books of record. Consequently, the law has proved thus far a nullity. A few physicians kept the sad catalogue which the law ordained, but no one came for it.

In former appeals to the legislature, the profession has perhaps been undeservedly placed in an unfortunate light. The attempted regulation of the drug business proved a very nauseous affair. The "Medical College" legislation excited a pamphlet war and the bitterest ill feeling. Here it has the opportunity of appearing in its true character, as the disinterested champion of what concerns the physical welfare of man, as the advocate of a system for the increase of knowledge among men. Let every physician, then, exert his influence for the removal of the obstacles to the successful operation of a registration law; and let us not witness another adjournment of the legislature before the great work is accomplished.

We design to reason elsewhere with the *cui bono* philosophers. Our present object is to call attention to an important point regarding the registration of deaths. The value of bills of mortality, even in places in which they are kept with the most scrupulous accuracy, is much diminished by the vagueness with which the causes of decease are recorded. This fact was considered by the Statistical Congress of all Nations, at its first meeting, at Brussels, at which that enlightened assembly declared, "that it was most desirable that a uniform nomenclature of the causes of death, applicable to all countries, should be adopted." It was voted, "that this nomenclature, the importance of which it was impossible to misunderstand, should be the object of ulterior discussion," and that two of the members, Mr. Farr, the registrar general of England, and M. Marc d'Espine, who holds the same office in the canton of Geneva, should prepare a plan for a nomenclature, to be submitted at the second session of the congress.

On the 10th of September last, the International Statisti-

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cal Congress convened at Paris, at the palace of the minister of commerce, who presided on the occasion. With the exception of the indignant protest of our compatriot, Mr. George Sumner, against the obstacles to free discussion imposed by the French government, we have heard nothing of the proceedings. We have, however, outlines of the reports of the eminent statists above mentioned, although we do not know what action was had on them. We publish the heads of these classifications, as interesting to nosologists.

Mr. Farr thinks that the most important elements for the nomenclature of the diseases of entire countries are derived from the old subdivision of maladies into: 1. Pestilence, or epidemic and endemic diseases; 2. Ordinary sporadic diseases; 3. Lesions, which are the immediate result of violence or of external causes.

His first class, epidemics and endemics (*zimotici, vel demici, vel morbi populares,*) comprises fevers, variola, the plague, the influenza, the cholera, and other diseases which have this in common, that they attack suddenly great numbers of persons placed in bad sanitary conditions. This class is divided into four groups: 1, miasmatic or zimotic diseases, of which *fever* is the type; 2, enthetic (from *enthetos*, implanted) or contagious diseases, communicable only by contact, puncture, or inoculation, such as *syphilis* and *glanders;* 3, dietetic diseases, originating in insufficient alimentation, such as *scorbutus, ergotism;* 4. parasitic diseases, of which *worms* furnishes a type.

The second class comprises constitutional diseases (*cachectici,*) divided into two orders: diathetic diseases, *gout, dropsy, cancer, gangrene;* and tuberculous diseases, comprising scrofula, phthisis, consumption, and hydrocephalus.

The third class the local diseases (*monorganici,*) embracing the diseases of the nervous system, of the organs of circulation, respiration, digestion, etc.

The fourth class comprises diseases of development (*metamorphici,*) which are designated by the names of still-born, premature birth, infantile debility, faulty conformation, diseases of dentition, chlorosis, puerperal and senile affections.

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The fifth class, violent death (*thanati*) comprises wounds and chemical lesions, poisons, suffocations, and the effects of mechanical forces.

Having thus separated diseases into five classes, according as they are epidemic, constitutional, local, or the result of faulty development, or of violence, Mr. Farr gives us this illustration:

"The statistician will form an idea of the first three classes by comparing them with the disorders produced in a perfect machine by electrical or chemical action or by long usage. The fourth class, to continue the same comparison, corresponds to faults of construction or a general dilapidation of the machine. The fifth class finds its analogue in an explosion which disintegrates the machine and finally arrests its movements."

The classification of M. Marc d'Espine, much more detailed than the preceding, comprises eight divisions: 1. Stillborn. 2. Death from original vice of conformation. 3. Death from old age or senile marasmus. 4. Death from violence, comprising suicide, homicide, juridical execution, and death from accidents. 5. Death by morbid accidents, comprising pulmonary and cerebral apoplexy, syncope, angina pectoris, internal hæmorrhage, sudden death. 6. Death from acute disease. [This division includes three classes, subdivided into a multitude of species—the acute part inflammatory diseases; the acute specific diseases; and the acute diseases with certain physiological conditions.] 7. Death from chronic disease, including chronic inflammations of organs, and chronic diathetic diseases. 9. Death from indeterminate causes.

Our registration law requires every physician and surgeon *to record at once the death of every person dying in this state*, upon whom he has *attended at the time of such death*, and, in addition to noting the age, sex and condition of the deceased, to state "*the disease or cause of the death*, so far as such fact can be ascertained." It is desirable, we say, that a uniform method should be adopted in the registration of the causes of decease; and to insure this we know of no plan which would be so effectual as the official recommendation by the

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state of some simple classification of diseases for general adoption—the classification to be published, and a copy of it placed in the hands of every physician and surgeon in the commonwealth.

We have quoted the reports to the Statistical Congress, with the hope of drawing attention to the subject, on which we invite discussion. We trust that physicians will exert themselves actively to perfect this and all other features of the registration act, and that the law will no longer be opposed by the leaden indifference which has heretofore rendered it nugatory.

Good Fellowship.

At this time, when the profession is assailed as it never was before—when quackery has acquired a stupendous development, and seeks, for the first time in the history of the world, to compass its nefarious objects by organized and associated effort—and when, it is to be feared, disreputable medical schools are sending forth multitudes to engage in the practice of medicine and surgery with insufficient qualifications and sordid motives, it is more than ever indispensable that the men of culture and attainment, whom we desire to have regarded as the true representatives of our profession, should be at peace and harmony with one another—and, if they cannot completely stifle those heartburnings and jealousies which are so likely to arise in an overcrowded and impoverished profession, that they should be silent in respect to them. We are proud to believe that professional bickerings are not more common in Virginia than elsewhere, and that we are almost entirely exempt from those underhand attacks which are said not to be unusual in some other localities. True, we have known of perfidious attempts to injure professional reputation by casting slurs on the politics, religion, birth-place, personal character, or method of practice of medical men. But such acts have been extremely rare. Their pepetrators, so far as we have known, have been a few malevolent individuals, who have incurred the general contempt of the profession. There is scarcely an honest man in our ranks, who would not lend a foot to kick them out, if the opportunity offered. These gentry, at the commencement of our editorial career, used to favor us with anonymous communications, containing malignant insinuations against different members of the profession, but became tired of the practice, as they learned that our journal was not designed for a gall-bladder to the body medical. Open quarrels are less

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infrequent among us. A misunderstanding occurs between two irascible brethren, both of whom perhaps, enjoy the esteem of a large number of medical friends, and thereafter themselves and their immediate associates are made thoroughly uncomfortable for months. It is to men of this character that we would appeal, and, pointing them to the innumerable foes which assail our profession from without, implore them to put confidence in one another, to heal and forget their dissensions, to let by-gones be by-gones, and to exercise mutual forbearance in the future.

We would quote on this subject the language of one of the best physicians, and perhaps the finest gentleman of which our profession can boast. At an anniversary dinner of the journal l'Union Medicale, in replying happily to a toast, Professor Rostan said, towards the conclusion of his speech, that he "desired to propose a toast in honor of the founder of the Union Medicale, which was not only a good journal, but a good action also, since its object is to bring together the members of the medical body, *which is the best mode of raising the estimation of our profession.* Its object is to induce us to love one another. If this end was attained, slander, disparagement and calumny would cease. We should not hear it said of our brethren that they were ignoramuses, that they killed their patients, that they treated them for diseases they never had, etc.—all of which allegations the public, always prompt to believe evil, greedily accepts without examination, and concludes that we are all assassins, noodles and charlatans. A poor way of elevating our professional standing! One cannot tell the evil he does himself in disparaging his neighbor. If instead of this we spoke of the fine qualities with which our brethren are endowed, the public would believe, what is quite true, certainly, that we were all savans, all skillful physicians, and estimable and learned gentlemen. Thus we should rise to the position which belongs to us. There is profit as well as virtue in speaking well of others. Let us then love one another, and to translate my language, by expressions savoring less of an homily and more appropriate to the occasion, let us all drink to the health of one another!"

Surgical Nomenclature.

Our correspondent from the "southwest," whom we know to be a most excellent practical surgeon, must excuse us for confiding to our readers a portion of the contents of his letter. Our friend writes in the guise of a medical notator and querist. He laments the inattention of our profession to the culture of humane letters, and bemoans his own want of classical knowledge. He descants on the mortifications to which this deficiency may expose the physician, using as an illustration the memorable incident in the "Ragland will case," in which a medical

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witness was unable to furnish the derivation of *pathognomonic* to his excellency Gov. Wise, himself, as everybody knows, a profound philologist. Our friend proceeds to inform us that a couple of students have lately entered his office, youths of enquiring minds, who frequently pose him by asking the origin of the terms he employs, and especially of the names by which he designates his instruments. (At this point, we were about to send for the embodied lexicon of our establishment, Dr. Pangloss, whom we consult whenever obsolete subjects are mooted; but as we read on we found the matter did not surpass our unassisted editorial capacity.) *Lithotome*, *tonsillotome*, and most of the modern coinings are as plain, our friend says, as the terms that end in *itis;* but the ancient surgeons, unlike the rhetoricians, seem to have had no method "in the naming of their tools." What is the meaning, for example, of *trochar*, *gorget*, *bistoury*, *scalpel*, *trepan*, and some other names occurring in the catalogue of the armamentarium? Our friend begs us to answer these questions, that he may silence the impudent youngsters, and to purchase him Dunglison's Dictionary, that such difficulties may not recur. We have sent an order to Mr. Randolph, in compliance with the latter request, and we will attempt to satisfy the former, though, as we have said, we have not our authority at hand.

Trochar is from the Latin *triquetrum*, a triangle, from the triangular point of the stylet. It was formerly called *trois-quart*, whence *trocart*, and *trochar*.

Gorget was a name applied to instruments exhibiting a groove, or channel, in the figure of a *gorge*. There was one used for passing the ligature in fistula-in-ano; another for conducting the forceps to the stone; then the latter instrument was modified so as to serve for dividing the prostate gland or dilating incisions therein. Hence the distinction into the *blunt* or *conducting* and the *cutting gorget*. The latter implement has almost usurped the name gorget. We must be permitted to say of it, notwithstanding the dicta of Physick and Dudley, and our friend from the "south west," that it is a most uncouth and dangerous weapon, of which the operator versed in surgical anatomy can seldom stand in need, and which the ignorant and inexperienced ought never to employ.

The bistoury gets its name from the town Pistori, a place once celebrated for the fabrication of these instruments.

Scalpel is from *scalpellum*, good enough Latin to be used by Cicero, and a diminuative from *scalpo*, to carve.

Trepan is equally good Greek; *to trupanon*, trypanum means simply a gimlet, and is from *trupao*, to perforate. It properly refers to the old instrument, still used by the French surgeons, resembling the carpenter's brace. The *trephine* is the improved hand trepan employed by British and American surgeons.

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We take leave of "southwest," thanking him for asking no harder questions. If his inquisitive pupils go on propounding interrogatories, we shall be forced to institute a section of notes and queries in the journal, and to intrust it to Dr. Pangloss.

Therapeutical Hints.

Cold foot-baths at bed-time are a valuable remedy in that sleeplessness caused by loss of blood. Brandy drinkers can generally bear very hot baths, the peripheral nervous system being dulled in them. In stomatitis, peppermint drops allowed to dissolve in the mouth produce a feeling of coolness, and are a good palliative. The tincture of cantharides, judiciously employed in retention of urine from cold, will often obviate the necessity of catheterism. In ganglions, acupuncture is an easily managed, little painful and generally successful mode of treatment, to be preferred to subcutaneous incision.—*Pauli.*

VARIETIES.

Medical News and Items.

SALTED MEATS.—M. Girardin of Rouen, has addressed a memoir to the French academy of sciences, on the nutritive properties of the salted meats of America, which, as our readers are aware, have been largely exported to France during the past year. We copy the conclusions of M. Girardin's essay from the Gazette Médicale, Nov. 1855:

1. The salt beef of America, although richer in nitrogen and phosphoric acid than fresh beef, and although nearly a double quantity of these principles is furnished for the same price, is a much less succulent, agreeable and nutritious article of food than fresh beef. 2. The salt lard imported from America is inferior in every respect to the lard of the country. 3. Our people have altogether given up the use of these salted provisions, not from caprice or prejudice, but from an experience of months and careful comparison. 4. It is useful to bring these facts to the notice of speculators, that they may furnish American meats in a different form; that they may be substituted with advantage for the butcher's meat, the high price of which almost deprives the laboring population of animal food.

A TREAT FOR THE BLIND.—Through the munificence of some liberal hearted citizens of Kentucky, the entire poem of Paradise Lost has been printed in raised letters.

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Case of a Foreign Body within the Cranium.—A correspondent writing from Bonne, in Rhenish Prussia, April 4th, relates the following: In the village of Rheindorf, near our city, M. Peter Klein has recently died at the age of seventy-five, a veteran of the French war, who, at the battle of Austerlitz, Dec. 2nd, 1805, was struck by a Russian ball, which buried itself in his left temple, above the rim of the ear, and remained there until his death—a period of half a century—without ever causing any inconvenience, or producing any change in his intellectual faculties.

After his death, his family, to whom he had bequeathed this ball, to be preserved as a memento of his campaigns, had it extracted by Dr. Backe of Bonne, who performed this operation with the assistance of a trephine, in such a manner that the ball remained surrounded by a ring formed by the bones of the skull. The ball on the side where it touched the brain was covered with a dense membrane, and upon the other with an investment similar to that elsewhere covering the head. The brain itself was uninjured, and no splintering of the bones of the skull was discovered, either in the neighborhood of the brain or elsewhere.

Scientific men assert that it is without precedent, that a foreign body placed within the skull, as was this ball, has produced no disturbance either in the physical or mental functions.—*Journal de Médecine, May* 1855.

Indian Graves in Concord, N. H.—In excavating recently, says the Congregational Journal, for a cellar of a new house, a few rods west of the dwelling of Richard Bradley, Esq. at the north end of this city, nine skeletons of Indians have been exhumed in a space about ten feet square. They are supposed to be the remains of some of the ancient Pennacooks who once inhabited this region, and probably have been buried at least a century and a half. Among these were skeletons of six children, three of whom were around that of an adult, supposed to be their mother, and one very large, measuring six feet and three inches. The bones of this giant were of remarkable preservation. The skull is very thick, the teeth in both jaws are entire, and all of them double. The skeletons were found enclosed in bark, in a sitting posture, with some of their long, black hair still preserved. The bones of the children were much decayed. Dr. Wm. Prescott of this city, has preserved the largest skeleton, which may be seen in his cabinet.—*N. Y. Observer.—Boston M. & S. Journal.*

Ravages of Cholera.—Cholera was still raging at Rio Janeiro, according to the last accounts, and was spreading to the back country. The first death was reported on the 16th of July, but the early cases were mild, and the disease did not assume a severe form until toward the end of August. The total mortality by the disease up to the 5th October last, was 1,776—1,207 men and 569 women.

Its ravages in some parts of Italy were truly frightful during the months of September and October. In the Duchy of Parma there were 13,372 cases, including 8,020 deaths; in Modena and Reggio, 11,396 cases, and 6,566 deaths; in the Grand Duchy of Tuscany, 49,618 cases, and 25,941 deaths—total, 40,527 dead. If to these be added the mortality of Lombardy, exceeding 50,000, and a similar number in the Roman states, and the total will show no fewer than 150,000 victims. Pestilence is more destructive than the sword.—*Med. News.*

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IORRHAGE,

TRANSLATED BY J. B. McCAW, M. D. RICHMOND, VIRGINIA.

In the cases which we suppose, there are yet no uterine pains. It is possible that he pains, for this medicine has the property of increasing the contractions when ke them when they do not exist.

e; then by the retention of the blood as well as by its own presence it irritates ges expulsive contractions. These dilate the orifice, and this dilatation will permit mination of the labor

ces. It prevents the increase of hemorrhage. We can however dispense with it ps the discharge. This latter course is perhaps the wiser; the more or less ten- choice of the one or the other procedure. 1st. WAIT if the hemorrhage does nishes; 2d. RUPTURE THE MEMBRANES, if we observe a tendency to an increased usefully preceded or followed by the administration of (a few doses of) ergot if

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AL OPERATIONS.

utions without which we should never attempt to

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d,) *should not have passed the orifice.* It is favorable if the membranes

mination of the labor, the preceding conditions (1st and 2d) existing—
e forceps.
ntation and position. CHOICE OF THE HAND. For the cephalic and
hand is not so important. *Take off your coat.* Anoint the DORSAL
erus. WAIT the absence of contractions.

DIFFICULTIES OF VERSION.

e right hand; if it does not adapt itself to the position of the fœtus, we
F THE VULVA. Not serious, (*introduce finger by finger.*) 3d. ARM IN
ou expect to practice embryotomy; even then the arm will be useful
ood letting, opiate injections, tartarized antimony, chloroform. If
event the arm passing up by the side of the head,) after having ascer-
nts and sometimes the position. *If version is impossible,* embryotomy.
OF THE HAND into the orifice; push slowly in the direction by which
N NOT FIND THE FEET. Endeavor to follow the lateral and posterior
oldly but prudently to the FUNDUS OF THE UTERUS and search about

enerally those caused by uterine contraction. Generally this stage is
THE HEAD ATTEMPTS TO ENGAGE WITH EITHER ONE OR BOTH THE
one hand and drawing slowly with the fillet outwards.

IPOSSIBLE TO TURN WITH ONE FOOT only, place a fillet on that foot
NE FOOT, if the other leg passes up before the trunk, hook the
mb, (its volume favors the easy passage of the head.) 3d. WHEN THE
ral movement, try and see to which side the back is most inclined to
HEAD, we must disengage them. Commence with the posterior arm,
onally for the posterior arm, depress for the anterior arm; then the
ded *as far* as possible on the external and anterior aspect of the arm,
body.) Always rotate the member towards the anterior plane of the
ide Pajot's thèse des Concours, "*Des lésions traumatiques du fœtus.*")
duce the index and middle finger of the hand, the palm of which best
of the fœtus and into the mouth; rotate the occiput behind the pubis.
M; HEAD FLEXED. Carry the back of the fœtus towards the back of
to the belly of the mother. If it is impossible to deliver, USE THE
D IN THE PELVIC CAVITY OR THE STRAITS. Push back the trunk
gers of the other hand, *en fourche,* on the nape of the neck, and carry
uraging her to bear down. If it is impossible to deliver in this way, the

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he pelvis will permit the passage of the instrument. It is favorable when the
l.

st and 2nd) existing, (inertia, hemorrhage, eclampsia, procidentia, &c.)

COMPLICATIONS AND DIFFICULTIES WITH THE FORCEPS.

FIRST STAGE.
INTRODUCTION AND PLACING THE BRANCHES.

1st. THE POSITION IS UNKNOWN. Make a direct application; (if the rotation of the head is not effected, it will sometimes happen that it will occur after introducing one branch or whilst it is between the blades, or the head will turn and the forceps with it.) If the movement of rotation is not produced, the direct application will be irregular, but, in general the delivery will be the same in this case. 2nd. WE CANNOT APPLY THE SECOND BRANCH. Withdraw the first and commence with the other. *In oblique applications*, there is always more difficulty in applying one branch than the other: If it is the anterior, (the *right* in the 1st and 2d positions; the *left* in the 3rd and 4th,) we commence withthis one, but to articulate, (the mortice being under the pivot in the 1st and 2d positions,) because the second branch is applied *always above* the first, in these two cases we are forced to uncross the branches. 3rd. THE EXTREMITY OF ONE BLADE MEETS WITH SOME OBSTACLE

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VIRGINIA MEDICAL JOURNAL.

THE STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL

COMBINED.

MARCH 1856.

ART. I.—*Surgical Reports.* By CHARLES BELL GIBSON, M. D. Professor of Surgery in the Medical College of Virginia.

Staphyloraphy.

THE operation for the relief of cleft or fissured palate is generally regarded as one of the most difficult in surgery, and many instruments have been devised to remove its embarrassments.

Unfortunately, these forms of apparatus are for the most part complicated and expensive, and therefore not within the comprehension or means of all aspirants to chirurgical honors and emoluments.

In the cases here reported, it is shown that the operation may be successfully performed by very simple instruments.

Master Joseph Taylor, 18 years of age, of Hanover county, Virginia, had congenital hare lip and cleft palate. The hare lip was relieved by an operation at an early period of life. The fissure of the palate extended through the symphysis of the palate bones, backwards and downwards through the

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uvula, which it divided into two equal parts. The width of the fissure was perhaps three-fourths of an inch.

Articulation and deglutition were both very difficult and painful.

Deeming it impossible to close the opening in the hard palate by any surgical means, it was determined to limit the effort to securing a union in the soft palate, thus converting an irregular, oblong fissure into an ovoid opening, which could probably be subsequently closed by mechanical contrivance.

I operated on the 27th of January 1852, assisted by the late Dr. C. P. Johnson and Dr. Peticolas. The instruments used were a narrow bistoury with long handle, a pair of long dressing forceps, Physick's forceps for deep arteries, and three fine curved needles, with ligatures. The patient was seated in a high arm chair fronting a window—his head supported. The mouth being widely opened, I seized the end of one-half of the cleft uvula, and holding it tense, incised from this margin of the fissure a strip an inch long and a line or more in breadth. After the bleeding had ceased, a similar strip was removed from the opposite margin of the fissure. Cold water soon cleansed these new surfaces from blood and mucus, and the patient was again ready. A needle with its ligature now being held in the artery forceps, was pushed through the velum at the upper end of the incised margin of the left side; then detached from the forceps by pressing the spring, and brought forward, held by the long dressing forceps. Again seized by the artery forceps, passed behind the right half of the velum, and brought through it in front, the ligature of course following it. Thus was the first suture placed.

More cleansing of blood and mucus was now required, occupying a few minutes, and then the knot was tied in the ligature, bringing the vivified margins of the velum gently in contact.

The remaining sutures, two, were placed at intervals of one-fourth of an inch in the same manner, and with less difficulty than the first, and each carefully tied.

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The operation thus concluded left an opening, comparatively small, consisting merely of the separation of the symphysis of the palate bones, whose margins were barely covered by thin mucous membrane, insusceptible of being detached or in any way of contributing to the closure of the cavity.

The patient bore the operation with remarkable fortitude; and although every step of its progress must have been painful and distressing in the extreme, he always returned to his position, and obeyed instructions with an alacrity truly astonishing to those who know the nature of such operations, and the coughing, hawking and spitting of mucus and blood which impede their progress.

On the 29th, forty-eight hours after the operation, the sutures were removed, and perfect union found to exist. On 10th of February the patient went home, with instructions to return to Richmond in the course of a month.

The after treatment of this case deserves notice.

Before commencing the operation, I had exacted a promise from the patient of implicit obedience to my directions. It was strictly kept. During the forty-eight hours before the removal of the sutures and the forty-eight succeeding hours, he neither spoke a word nor swallowed a morsel of food—not even water—but remained in bed and expressed his thoughts upon a slate to an attendant, who never left him. Many times a day a little cold water was gently thrown upon the velum with a small glass syringe, and always proved most grateful.

On Master Taylor's return to town, he was placed in the hands of Mr. J. G. Wayt, one of our most intelligent dentists, who took a cast of the roof of his mouth, and adapted a gold plate to the opening in the bony palate, which completely closed it, and together with the previous union, restored the patient's articulation and deglutition to a nearly normal standard.

Case II.—Master J. W. Johnson, 16 years old, of Wythe county, was sent to me by my friend, Dr. Crockett of Abing-

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don, with fissure of the palate, resembling exactly the case above reported. Hare lip had also existed, and had been relieved by operation early in life.

On the 2nd July 1855, assisted by Drs. Wellford, Peticolas and Mayo, an operation was performed, in all respects similar to the one just described, and the same after treatment pursued. The sutures were removed on the fourth. Perfect union occurred, and the parts remained consolidated until the 5th day after the operation, when from excessive coughing, brought on by carelessness of a servant in sweeping the chamber with the window open, the union was suddenly ruptured, and the fissure rendered as bad or worse than before.

This accident was of course much lamented, but my young friend was not discouraged; and after remaining at home during the rest of the summer and autumn, returned to me in November.

On the 6th of November I again operated, assisted by Dr. Watson and some medical students. The sutures were removed on the 8th, and union was found to be perfect.

Three weeks afterwards, a gold plate was fitted to the opening in the hard palate, and he returned home, very much improved in speaking and swallowing.

These cases are interesting to the profession in many respects. They show that an operation, once regarded so formidable as to deter many practitioners from attempting it, may be successfully performed by the simplest means. That complete abstinence from food, solid or liquid, may be borne, by an exertion of the patient's will, for days together, without distress. That failure may be due to causes unforeseen, and therefore not guarded against. That one failure does not prevent the success of a second operation. And lastly, that although the wide separation of the bones forbids the closure of the chasm between them by plastic surgery, yet a combination of surgical and mechanical principles may in very bad cases result in the complete restoration of the functions which this hideous malformation so seriously impairs.

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ART. II.—*On Placenta Prævia.* By JOHN P. METTAUER, M. D. L. L. D. of Virginia.

IMPLANTATION of the placenta over the os uteri may be central or marginal, and in either case will pretty uniformly be the cause of hæmorrhage, as the uterus expands during the advancing periods of utero-gestation. Where central implantation exists, the safety both of mother and child will be seriously jeopardized, while a marginal attachment chiefly endangers the life of the fœtus, and not necessarily that of the mother, though by neglect and improper treatment, she too may be placed in imminent peril by it.

It is generally at or about the seventh month of gestation that placenta prævia begins to endanger from hæmorrhage; and this is due to the more rapid and extensive expansion of the cervix uteri, from this period, in enlarging the uterine cavity, to accommodate the increasing size of the contained ovum. In some instances, however, no serious danger threatens until labor commences at the full term.

In central implantation the placenta is attached more or less centrally over the os uteri, and is decidedly the most important and dangerous deviation, while the danger diminishes as the attachment varies from this to the marginal.

The hæmorrhage attendant on placenta prævia from the seventh month, and before labor commences at the full term, is due chiefly to the separation of the placenta around its circumference, and the consequent rupture of its vessels there, caused by the increasing dilatation of the cervix uteri at this period; while that occurring with the commencement of labor is caused by the separation of the placenta, and rupture of the utero-placental vessels around the os, as this opening dilates to give exit to the uterine contents.

In the first, the hæmorrhage is less violent and menacing than that connected with separation of the placenta from the os uteri, because the separation is gradual in the former, and the vessels, that are ruptured from time to time, smaller

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and more delicate. Here the vessels are ruptured only as the cervix expands; and as this dilatation is slow and gradual, the separation of the placenta and rupture of the connecting vessels will be slow and gradual also, and the consequent hæmorrhage comparatively moderate.

When, however, labor commences, the os uteri dilates more or less suddenly, and the separation of the placenta, if the case be one of central implantation, will be proportionally sudden also, and the consequent hæmorrhage must necessarily be sudden and impetuous in greater or less degrees.

If the hæmorrhage appears near about the 7th month, it will in all probability result from the separation of the placenta at its circumference, as no other portion would be likely to be detached at this period of utero-gestation; and it would be moderate, because, as already stated, the placenta must be more or less gradually separated, then, from the uterine surface, by reason of the comparatively slow expansion of the corresponding wall.

But should it set in with the accession of labor, if the views already expressed are correct, it would be both sudden and impetuous, from the sudden separation of the placenta, and rupture of the utero-placental vessels at or near the os, chiefly, if not exclusively the result of sudden dilatation of the os uteri, as preparatory to the expulsion of the fœtus. And such will be the case, also, should a border of the placenta only be connected with the os uteri.

These divisions of placenta prævial implantation, if carefully studied in practice, will enable us to determine at once the character of the hæmorrhage, both as respects the seat of the vessels yielding the blood, and its danger to the mother and fœtus.

In many instances, uterine hæmorrhage occurs during pregnancy, at or after the 7th month, that causes not a little alarm with females, and much perplexity with practitioners in determining as to its precise nature.

The discharge of blood often, in some of these examples,

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will be quite free and menacing for a few moments, but will very soon greatly abate, or cease for some hours entirely, leading the physician to believe that there will probably be no return of it.

After a while, however, and without any known cause, it recurs, and pursues the course of the first attack; and again abates, or ceases entirely; and in this manner it may return and abate after intervals until labor sets in at full term; or it will cease altogether before that time.

This description of uterine hæmorrhage must either be due to central implantation of the placenta over the os uteri, or a marginal attachment may exist, with the border of the placenta extending beyond the os uteri sufficiently to occlude it. It may also be due to detachment of a portion of the chorion from the corresponding uterine mucous lining, by an accidental blow on the abdomen, or by a fall or severe jolt.

If it result from placenta prævia, the probabilities are against the complete arrest of the hæmorrhage until the attendant pregnancy terminates, and in favor of the return of it in an aggravated and menacing form when labor sets in. But should it depend on partial separation of the ovum, in a majority of cases spontaneous arrest of it would follow, and a cure be effected by proper treatment, with little danger of recurrence, unless brought on by exposure of the woman to causes similar to those that induced it at first.

A sudden and profuse discharge of blood from the vagina, as labor commences, or as the os uteri begins to dilate, could not fail to lead to the belief that there was implantation of the placenta over the os uteri, and that the hæmorrhage resulted from the rupture of the utero-placental vessels, caused by the sudden detachment of the placenta from the os as it is forced open by the uterine contractions, constituting labor in its first or preparatory stage.

In these cases it will invariably be the duty of the obstetrical attendant to examine carefully into the condition of the os uteri, per vaginam, in reference to implantation of the

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placenta over it. If the hæmorrhage is profuse, little difficulty will attend the introduction of the index finger of either hand into the os uteri to a sufficient depth to ascertain if the placenta is situated upon it or not, as that opening is always more or less dilated by the escaping blood. The examination, however, should be conducted with gentleness, taking care, as the finger is introduced, not to force open the os violently, nor to employ undue force with it against the placenta.

If the case is ascertained to be placenta prævia, the cause and nature of the hæmorrhage will be satisfactorily declared, and a course of treatment must be adopted accordingly. But if no placenta can be found to occlude the os uteri, and instead of that structure the delicate and thin membranous envelop of the fœtus presents, it will be fair to presume that the hæmorrhage is due to accidental separation of the ovum from the uterine surface, which we know is generally harmless and easily corrected—nay, most frequently, it subsides spontaneously, if the woman is still and quiet in bed for a few days.

In the treatment of pregnancy and labor, complicated with implantation of the placenta over the os uteri, great difficulty has always been experienced, whether the attendant hæmorrhage commences early or at full term; and perhaps no question in obstetrical therapeutics has given rise to greater contrariety of opinion or of medication. It is not the writer's intention to notice these discrepancies, as he could not examine them fully in a paper like this, designed chiefly to explain his individual methods of treating placenta prævia, and through the pages of a monthly medical journal. Having had considerable experience in the management of this menacing and formidable obstetrical hydra, he proposes merely to present a condensed transcript of that experience.

In hæmorrhage connected with placenta prævia, from the 7th month to the close of the 8th, it will not generally be necessary to do more than maintain the bowels in a soluble

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state by gentle cathartics, such as the bitart. potass. Seidlitz powders, and the like; to detract blood if the habit is plethoric, or the pulse strong and excited; to enjoin absolute rest in the recumbent posture; to restrict the diet; to guard against nervous excitement; to allow free ventilation of the woman's apartment, and cold drinks or ice; to require the person to be loosely dressed, and to be kept cool; to use narcotics in combination with astringents if there is much nervousness, as internal remedies, or astringents alone if narcotics are not demanded, especially acetis plumbi sulp. alum or tannin; and cold, wet cloths over the hypogastrium and vulva.

Sometimes it will be necessary to have recourse to the tampon, especially if the woman is delicate and the hæmorrhage very profuse; and when employed, it should be carefully applied to the os tincæ, and steadily and firmly pressed against it until the hæmorrhage ceases.

Even after the external appearance of blood is no longer visible, it will be safest to continue the pressure upon the tampon for some hours; and when the pressure is to be discontinued, it must invariably be gradually done, and the instrument suffered to remain.

The writer has often witnessed the return of the flooding by too suddenly removing the pressure from the tampon, and from the premature withdrawal of the tampon also. The best tampon can be made of raw cotton enveloped in fine linen or kenting, so as to form a firm ball; and the surface to be applied to the os tincæ, before the envelop is put on, should be well saturated with a powder of equal parts of sulphur, alum and tannin. By using a square piece of soft old linen of proper size for the envelop, it can be put over the medicated cotton so as to form a handle beyond the ball, that will be found convenient in the introduction or removal of the tampon; and to render the ball firm, the gathers of the envelop should be tied firmly close up to the corresponding surface of the cotton with a strong thread.

In numerous instances, the writer has succeeded in re-

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straining uterine hæmorrhage, in cases of placenta prævia, by the energetic employment of the plan of treatment that has been briefly sketched, and women have been conducted thereby, in comparative safety, to the close of pregnancy. It is true, hæmorrhage, connected with malposition of the placenta, is not generally attended with very great danger in the 7th and 8th months of pregnancy, especially the 7th, as already stated, but it should always be regarded with solicitude, because it is sometimes fatal. When it complicates labor, however, it is indeed a formidable accident, demanding decision and the most exalted skill, in many cases, on the part of the obstetrical attendant, to avert the dangers that peril the life both of mother and infant; and it is the form that this paper is designed chiefly to consider. A labor thus complicated is distinguished by the discharge of blood from the vagina, more or less profusely, with the commencement of uterine pain; but the hæmorrhage is most free and impetuous as the os uteri becomes dilated, and particularly so, if the dilatation is sudden; and it is the sudden separation of the placenta from the os and cervix uteri, and the consequent rupture of the connecting vessels, that cause the hæmorrhage, as has already been stated. In many cases, these pregnancies are attended with occasional discharges of blood from the 7th month down to the commencement of labor, that are restrained by appropriate treatment; but, although they are menacing in a high degree now and then, the hæmorrhage is far less impetuous than that ushered in by the accession of labor.

In treating this complication of labor, the aim should be to save both mother and child, if possible. But if this is impracticable, and one must be sacraficed, every consideration of duty and humanity demands that the fœtus shall be the victim.

When hæmorrhage sets in with labor, whether impetuous or not, the safest and most reliable means of restraining it is the tampon; and it should be resorted to without a moment's delay; and formed and applied as already suggested,

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only that the extract of belladonna must be used with the astringents in medicating it, and in quantities varying from twenty-five to forty grains. This instrument, as already stated, should be carefully and firmly applied to the os uteri, so as to prevent the farther escape of blood from the uterus, and to cause a coagulum to form in the os and over the separated portions of the placenta. By the addition of the belladonna to the alum and tannin, two important ends may be simultaneously attained; that is, the coagulation of the blood around and in the orifices of the ruptured vessels, and the prompt and free dilatation of the os uteri.

In many cases the hæmorrhage will be promptly and completely arrested by these measures; but if not completely stanched, it will often be greatly moderated, so as to allow time for the safe and full dilatation of the os, and spontaneous delivery of the fœtus.

If, however, the hæmorrhage proves obstinate, and from the blood already lost, seriously endangers the life of the woman, it will not be safe and proper to trust farther to the tampon alone, but immediate resort must be had either to delivery by introducing the hand into the uterine cavity and turning, or to forcible separation of the placenta from the uterus with the index finger passed between them.

If the hæmorrhage has been very profuse and exhausting in its effects, whether of long or short continuance, attempts to deliver would not be justifiable or safe.

But should the obstetrical attendant see the case before such prostrating losses of blood had taken place, and the os uteri could be entered without the employment of undue force, delivery by turning or with the forceps might be promptly resorted to. But to attempt delivery after the woman has become greatly enfeebled by the floodings, would seriously endanger her life. Indeed, it would be murderous, in a majority of instances, as the process requires more time for its completion, even when executed in the most dexterous manner, than could be safely consumed while the flooding continues.

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In numerous instances, attempts to deliver, or even its accomplishment, have proved fatal under these circumstances, when life might have been perpetuated by a different procedure. True it is, some few women of uncommon vigor of constitution have escaped with life; but such examples are remarkable exceptions, and do not in the least weigh against the plan here advocated.

If the case forbid all attempts to deliver, the second expedient that has been suggested must be adopted, and without a moment's delay; that is, the separation of the placenta from the os and cervix, by forcibly and quickly passing the index finger between them, so as completely to detach the placenta from the uterine surface.

This operation causes considerable but temporary increase of the hæmorrhage, and should be executed with the utmost despatch, or the woman may sink from the loss of blood before it is accomplished. As soon as the placenta is detached, the medicated tampon, without the belladonna, must be applied. And to save time, that instrument should invariably be previously gotten ready. By carefully pressing the entering surface of the tampon against and partially into the os uteri, and firmly holding it there with the fingers, the hæmorrhage will instantly cease; and after a short time thus restrained, there will be very little danger of its recurrence, even if the tampon should be removed, which, however, must not be attempted: it must invariably be forced away by the uterine contractions upon the renewal of labor.

When the case is a marginal implantation of the placenta over the os and cervix, and delivery is forbidden, by reason of the previous loss of blood, and great prostration of the woman's strength, it will be necessary also to resort to the expedients just described. In some of these cases, however, when the placenta barely or imperfectly occludes the os uteri, it will only be necessary to detach it partially; and the writer's experience has led him to adopt and to advise the practice of separating that organ on its occluding border to a line a little beyond the opposite half of the os uteri.

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This separation very often, aided by the medicated tampon, will completely or greatly restrain the hæmorrhage, and at the same time afford a chance for the preservation of the fœtus.

After the hæmorrhage is restrained by the means which have been suggested, a question arises as to the propriety of speedy delivery by artificial means. Should there be very great prostration of the system from the flooding, artificial delivery must not be attempted until the energies recuperate, as manifested by a well developed and equable reaction. In numerous melancholy instances, women have perished by being delivered too soon after the arrest of profuse hæmorrhage from placenta prævia, especially before the powers of the system had reacted sufficiently to restore a well balanced circulation. As long as there is no hæmorrhage, and absence of uterine contraction, attempts at artificial delivery should never be made. There will be infinitely less danger in delaying delivery in such cases several days, if reaction is so long deferred, than from an early resort to it. Inflammation might supervene from the detention of a dead fœtus 24 or 36 hours after the arrest of the hæmorrhage, causing its death; but this possible contingency would not bear a comparison with the almost certain danger of death to the mother, should she be delivered before reaction takes place to a sustaining extent.

The writer has known women to die, in several instances, almost at the moment of delivery, who had previously flooded most profusely from placenta prævia, but had ceased to do so as soon as the placenta was detached. In these cases there was great prostration of the system, manifested by difficult respiration; absence or alarming depression of the radial pulse; tinnitus aurium; moaning; restlessness; and a tendency to swoon from the slightest change in the posture of the body; and, though delivery was not attempted or accomplished for more than ten hours after the hæmorrhage had been arrested, there was not the slightest tendency in the system to react; and the woman

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died the instant the stimulus of distention was removed from the uterus and abdomen. In such examples of placenta prævia, the safest and most rational mode of procedure is, to employ such restoratives and incitants as may be demanded to re-excite the flagging operations of the system, and to wait patiently until reaction is fully restored, or until spontaneous delivery takes place, which often occurs with the re-establishment of a well balanced circulation, and is seldom fraught with danger, unless improperly managed or officiously interfered with.

Should utero-peritonitis threaten, even before delivery is accomplished, purging the bowels impressively once or twice will effectually avert it, and not only without increasing the depression of the vital actions, but actually re-exciting them. In some most menacing and exceedingly perplexing cases of this kind, the writer has resorted to purging with the happiest results in promptly exciting the uterus into vigorous and effective contractions, speedily resulting in delivery, and in the removal of the symptoms threatening inflammation also.

It is important after every delivery to have the abdomen supportingly bandaged; but in the cases now under examination it should be done with compressing force, and continued until every menacing symptom subsides, unless contraindicated by increasing abdominal tenderness, and when to be discontinued, it must invariably be gradually done.

The views advocated in this paper, in regard to the treatment of labor and hæmorrhage, in cases of implantation of the placenta over the cervix and os uteri, have been long entertained by the writer, and their correctness and safety often verified in his intercourse with cases complicated with such malpositions, or, as usually denominated, placenta prævia. In more than thirty-five cases that have been treated by him, only two unfortunate results followed; and the treatment now pursued by him, and commended through this paper to his brethren, was suggested by those two unfortunate cases more than 25 years ago, while he was a young practitioner of medicine.

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These views, however, are not peculiar to the writer, although original with him; and it is gratifying to find that they are entertained, in the main, also by some of the ablest and most successful as well as prominent obstetrical practitioners both of this country and of Europe, with Professor Simpson—generally conceded to be the highest authority at the present time in midwifery—at the head of them.

Prince Edward C. H. Va. Feb. 10, 1856.

ART. III.—*Practical Treatise on Strictures of the Urethra.* By J. F. REYBARD, M. D. Translated from the French. By GEO. C. STARKE, M. D. Hicksford, Virginia.

The Tissue which forms Stricture.

THE existence of the abnormal tissue of stricture I regard a fact of the greatest importance. The evidences to prove it are numerous. Autopsies, clinical observations and theory confirm this assertion.

MM. Velpeau, Cruveilhier, Lallemand, Civiale and Amussat have cited many examples of the fibrous or fibro-cartilaginous nature of this tissue, and it appears to me that M. Cruveilhier has understood its nature better than the others. He says, "Its fibrous character cannot be doubted. I have never found strictures of any other nature. There is disappearance of the mucous membrane at the constricted point, and disappearance more or less complete of the erectile tissue. Having before us the fibrous character of stricture, if we examine the cause which produced it, we see that its formation can be explained only in two ways.

"1. By chronic inflammation of the mucous membrane.

"2. By ulceration.

"Our knowledge of the pathological anatomy of the urethra in blennorrhagia is not sufficient to determine this question positively. I believe that stricture is the result of ulceration; for if attributed to inflammation, it would be

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difficult to understand why the effects of that inflammation should almost invariably be limited to a single point of the urethra."

I differ with M. Cruveilhier in one respect only. He, as many authors, considers stricture as being formed most frequently by a morbid tissue with chronic engorgement, or induration susceptible of resolution, and which is capable of dilatation. He thinks that the tissue takes the fibroid or heterologous nature only, when the organic coarctation is far advanced. Its transformation then is not certain, and sometimes might not occur. According to my opinion, the newly formed tissue is not only found after the stricture has long existed, but it constitutes all strictures without distinction, and is observed as soon as the inflammation commences to subside.

To support my opinion, I give some post-mortem observations which I made on subjects in the amphitheatres of Lyons. The majority of the subjects had been carried off by other diseases, and in these cases the strictures were, to express myself clearly, in their normal state, not complicated by rents, or inflammation, consequently more favorable for study than if the persons had died during the treatment of such lesions.

1. *Autopsy.*—Man, 50 years old, died of phthisis. Eight centim. from the meatus there existed a circular stricture three to four millim. long, its orifice admitting a sound of three millim. By a longitudinal incision we discovered all the tunics of the urethra to be involved by the stricture. The tissue presented a fibrous appearance, was whitish, having the usual thickness of the urethral walls. It was hard, resisting and inextensible. The color of the mucous membrane was little changed, but the membrane participated in the alteration, and was so firmly united to the subjacent tissue that it could not be dissected from it. At the point of obstruction the breadth of the walls was diminished one-half.

2. Subject about 60 years of age. Eleven centim. from the meatus stricture, two millim. long, orifice admitting a bougie three and one-third millim. It had the appearance

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of a valvular stricture when the walls of the urethra were divided longitudinally one centim. in front and behind the constriction. The abnormal tissue was dense, contracted, very resisting, of a fibroid aspect, but had not the usual white color. It was thin, and confined to the internal membrane.

3. Subject 50 years, died of typhoid fever. Four and half inches from the meatus, stricture two millim. long, which would receive a bougie 4 millim. It was thin, fibroid, resisting, perfectly inextensible, white and smooth, confined entirely to the internal membrane. If cut or scratched, there was a kind of crepitation, or rough friction sound heard.

4. Subject 60 years, died of aneurism of heart. Circular fibroid stricture two centim. from the meatus, about four millim. long. It was situated exclusively in the mucous membrane, which was whiter and more smooth than in health.

5. Examination of a man who entered the hospital in 1832, to be treated for strangury, and died in a few days from spontaneous perforation of the stomach. At the subpubic curvature of the urethra, there was a stricture which admitted with difficulty a grooved director. The canal being incised longitudinally, presented at the constricted portion a circular thickening of the mucous membrane, commencing and ending in an insensible manner. Each half of the divided canal resembled a spindle split lengthways, large in the middle, and diminishing in size at each end. The external surface was not more prominent than the internal, so that the cylinder which formed the obstacle projected as much into the canal as externally. The mucous membrane adhered firmly to the altered tissue, and could not be dissected from it. The altered tissue was of a yellowish white color, firm, resisting, slightly elastic, and easily torn. It had not the appearance of being composed of fibres, but resembled an albuminous substance deposited into the sub-mucous areolar tissue. This case I borrowed from M. Lallemand, which is recorded in his book on stricture.

6. Examination of a physician who died in the service of M. Robert in 1852, of diseased heart. It was witnessed by MM. Robert, Leroy and Mercier. The stricture, situated in the region of the bulb, was 3 centim. long, having a central orifice through which a bougie 2½ millim. passed. Through the integuments the stricture was distinguished by its hardness, which contrasted strongly with the remainder of the canal. The indurated portion of the urethra being detached

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from the surrounding tissues, made no projection externally, and was no larger than the healthy parts of the urethra. A longitudinal section revealed the existence of a tissue-like cartilage, but not so firm. The several layers composing the walls were closely united, and could not be dissected apart. In some points were discovered the large cells of the corpus spongiosum, the mucous membrane very adherent, and covered with white silvery spots, though not so completely altered as is generally the case in fibroid stricture. The morbid tissue had the same shape as that last spoken of, large in the middle and small at the extremities. The most important fact in this observation is, that although the stricture could be felt through the integuments, by dissection there was not found any thickening of the urethral walls, which, instead of being thicker, were only more dense. They had become hard, but not tumefied.

7. Man, 42 years old, entered Hotel-Dieu of Paris in February 1845.

March 1.—M. Blandin recognized a stricture, very much contracted in the membranous portion of the urethra, admitting a bougie 2⅔ millim. There were two other strictures in the bulbo-spongy region, less marked. A bougie No. 3 was introduced and left in the canal two hours.

March 2.—Introduction of same bougie.

March 3.—Patient complained of general uneasiness, loss of appetite, want of sleep, and heat about the head. The skin hot and pulse more frequent. Diet, ptisan, &c. Bougie not introduced.

March 4.—Condition as yesterday; expression of countenance somewhat wild. Same prescription.

March 5.—Absence of fever. At my request Mr. Blandin incised slightly the first stricture, which was not considerable, and situated one and a half inch from the meatus; no hæmorrhage. Some hours after the operation, the patient was taken with rigors, and died in the evening.

At the examination of the body, there was nothing discovered but congestion of the brain, and we think the patient died of an attack of pernicious intermittent fever. The urethra was opened longitudinally on its superior surface. On the inferior surface of the canal at the first stricture, we discovered a longitudinal wound eight or ten millim. long, caused by division of the mucous membrane. The wound did not involve any other parts—that membrane

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was not changed in color or thickened. In this portion of the canal we could not by inspection or minute dissection discover any alteration of the tissues, but we were sure a stricture had existed, because the mucous membrane was not half as extensive there as it was in front and behind that point; besides, it was very inextensible.

The second stricture was formed by two bridiform constrictions one centim. apart, situated in the bulbo-membranous region. The mucous membrane there was only affected, and its fibroid aspect more apparent—it was more smooth and polished than in health, inextensible, and less extensive than in other parts.

From this observation, we see there are some strictures difficult to detect even on the cadaver. These are the cases, without doubt, which have made authors admit the existence of simple spasmodic stricture when there was organic constriction really.

8. Autopsy made in presence of the committee for the prize of Argenteuil, 1845. There were many strictures. The one nearest the meatus was fibrous, and the alteration limited to the mucous membrane. The second, 17 or 18 lines long, occupied the bulbous and a portion of the membranous region; fibro-cartilaginous, whitish, having the consistence of cartilage, and without doubt, of the same organization, about one millim. thick, developed in the internal membrane, which was not increased in thickness. The tissue was solid, resisting and inextensible. It was impossible, after many attempts at dilatation, to introduce bougies larger than those used the first day. Although this tissue was inextensible, it is probable that it did not possess the power of contracting, and could not constrict the canal gradually, as happens with strictures whose walls are formed by a fibrous tissue less advanced in its organization.

In the canal of the stricture there was a little band of same color and nature as the stricture. It was three millim. broad, and 18 or 20 millim. long, lying obliquely in the canal of the stricture, having both ends joined to the inferior wall.

After one has examined numerous strictures, he must have noted how rapidly the different methods of treatment have

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accelerated the development of the abnormal tissue. Every cause of irritation accelerates its organization, and we add, that the progress of stricture is in direct proportion to its state of organization. Thus, patients, who have been treated by dilatation, and who use bougies to maintain a certain calibre, are compelled to introduce them at shorter intervals, as the time increases from the date when first used.

I have made many other examinations of stricture, and always found the morbid tissue as I have described it—sometimes limited to the mucous membrane, again invading all the tissues of the urethra at the same time. Those of my confrères who have had occasion to make autopsies of this kind, assure me they have found the tissue of stricture in the state indicated, and have never found it in a state of thickening or chronic engorgement.

It is not reasonable to admit that engorgement of the urethral walls causes stricture, for we know that the blennorrhagic discharge, a sign of acute or chronic inflammation, ceases almost entirely before the difficulty of urinating comes on, which difficulty reveals the existence of stricture. If engorgement of the walls constituted the coarctations, should not these lesions, instead of being developed and gradually increasing after the blennorrhagia ceased, follow an inverse march? There is, then, something more than an acute or chronic inflammatory tumefaction to produce stricture.

How is the tissue of stricture formed?

It is formed as an ordinary cicatrix, and not by degeneration or transformation of the urethral walls into an abnormal tissue. During the inflammatory stage, principally towards its decline, the organizable liquids, exuded into the inflamed areolar tissue, coagulate, condense, undergo a vital process, and are organized. Circulation is established by new vessels, and this heteromorphous production is endowed with life. At the same time the subjacent tissues, crowded and compressed by it, deprived more or less of their nutrition, are rapidly atrophied, and finally completely disappear.

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In order that this substitution of tissue may take place, it is not necessary that suppuration occur. It suffices that there is thrown out, into the sub-mucous areolar tissue and cells of the corpus spongiosum, a considerable quantity of lymph to compress the surrounding tissues. This explains why blennorrhogia without ulceration sometimes produces stricture.

The new tissue is generally thinner than many have stated, and is not in a state of chronic engorgement. Its thickness is greatest immediately after the inflammation, before interstitial absorption commences, and is always less than the thickness of the tissues which it replaces.

This tissue is more consistent than the urethral tunics in health, and we can often detect the stricture of the canal, by feeling the hardened urethra externally.

The tissue of stricture possesses a property, that of contraction, in virtue of which organic coarctations become shortened, as do common cicatrices, and thus narrow the canal in a slow but progressive manner. This property of the new tissue would seem to be denied by those in favor of dilatation as a method of cure, because in admitting the curability of stricture by dilatation, do they not deny the power which it possesses of contracting in a slow and progressive manner? Or if this quality is admitted, do they not place limits beyond which it cannot go? To say stricture may be cured by dilatation, is to admit that its power of contracting is not inherent, not essential, and that it may be paralysed or destroyed. I regard the tissue of stricture as being contractile so long as it exists; and it is as much impossible to destroy this property as it is to change its nature.

All authors agree that stricture has a progressive march, and the constriction increases as they grow old; but the majority of persons think its progress is owing to the inflammation which first caused it—that stricture increases because chronic inflammation augments in surface and depth the engorgement of the diseased walls. In speaking of urethral *brides* or bridles, M. Civiale says, they appear not to remain

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stationary, either from a deposit of coagulable material in their substance, or because the submucous cellular tissue becomes hypertrophied. The coarctation increases in extent, as we discover from the difficulty in urinating.

M. Brodie is of the same opinion. He says, new changes take place by the progress of the disease, which, instead of being limited to a small portion of the canal, as it is in the beginning, extends on each side of the point first attacked.

M. Lallemand, in relating a case which he found difficult to destroy by cauterization, says, "We must conclude that indurations of the canal increase, as they become old, in thickness, firmness, and in length."

I believe that every stricture, from its commencement, has its maximum of length and thickness, and that time, far from increasing their extent, diminishes them in every sense. The constriction of the canal, which increases, is not due to any appreciable anatomical modification of the tissue, but to the progressive contractile nature of that tissue—that this contractile power is different in different strictures, and is greatest generally in those of long standing.

Elasticity of the new tissue is the property which causes strictures to return, after dilatation, to their former size, as a spring, or as the yellow elastic tissue would do if put on the stretch. This elasticity is inherent in the tissue of coarctations, which we call *dilatable*, and to it is due the compression often made on sounds during dilatation.

Extensibility.—The facility with which we can dilate recent strictures removes all doubt as to the extensibility of the tissue of stricture: it is sometimes so great as to permit the passage of sounds, with little or no resistance. In such cases catheterism induces us to believe the canal clear, when there are other evidences of stricture. Before learning the great extensibility of this pathological tissue in certain cases, I was not disposed to believe a stricture could exist in a canal which admitted a sound seven millim. although there were symptoms which indicated it.

In 1842 a young naval officer came to me, who had been

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treated several years for vesical catarrh and spasmodic stricture. In 1840 he was cured of a second blennorrhagia, and since that time the symptoms of a new disease manifested themselves, and he was treated for stricture. When I saw him he had frequent desires to urinate. The stream of urine was feeble, flattened and spiral; perinæal pain increased by walking or sitting, and dull hypogastric pain. The urine being ammoniacal and glairy, with the hypogastric pain, made me suspect a catarrhal affection of the bladder. On seeing the patient urinate, I concluded immediately he had stricture, but after introducing an exploring bougie of Ducamp seven millim. I was of the opinion of those who had not succeeded in discovering a stricture. Notwithstanding the resistance which the sound met with, and the pain occasioned by its traversing the spongy and membranous regions, it did not appear that a coarctation could exist in a canal which admitted a sound of such volume. I told the patient he had no stricture; advised antiphlogistic treatment with mild injections, and sent him to Vichy. The use of the water for two seasons was beneficial; pains left him; the urine was less glairy, and the desire to pass it less frequent. The difficulty in micturition, however, persisted. The form and volume of the jet induced the idea of a stricture more advanced than before; and though the desire to urinate was not so frequent, it required a longer time to expel it than formerly. Such was his condition in 1844, I attempted catheterism again, but with a flexible bougie á boule three and two-thirds millim. and discovered a stricture about one centim. long at the bulb. The walls were still so extensible that a conical sound eight millim. could be introduced.

From this case we see that the tissue of stricture, although it may be stretched and distended by dilating bodies so much as to remove all suspicion of organic lesion, offers, nevertheless, sufficient resistance to diminish considerably the urinary jet. We conclude from this observation, that strictures very dilatable, yet which diminish the jet of urine, are not less grave than those which are extensible only in a slight de-

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gree. Both give rise to disorders of the urinary functions, and both demand, contrary to the general opinion, curative treatment. Upon this consideration, I recommend earnestly urethrotomy as a general method for the cure of stricture.

I regret that the committee which gave me the prize d'Argenteuil, restricted urethrotomy to the cases in which dilatation was unsuccessful. I regret still more, that the committee did not explain itself more fully when restricting urethrotomy to strictures refractory to dilatation. Does the restriction apply to all strictures which are not permanently cured by dilatation, or to those which do not yield to the dilating influence of sounds? In the first hypothesis, urethrotomy would be the method of treatment in all cases, for the efforts at dilatation cannot change the contractile nature of this heterologous tissue. In the second hypothesis, the operation would seldom be performed, because there are very few coarctations which cannot be dilated in some measure. In my opinion, the operation of urethrotomy is demanded not only in strictures where dilatation is difficult and painful, but in those too contractile and elastic to retain the degree of dilatation produced by sounds. In a word, in all strictures which are not supple enough to permit the separation of the urethral walls by the stream of urine.

The extensibility of strictures presents great varieties, and it is difficult to appreciate the cause of it. I have often observed that the tissue was less resisting at the commencement than at the end of the dilating treatment. In other cases I have observed the tissue extensible to a certain degree, beyond which dilatation was painful and impossible. It is to the extensibility of this tissue that we must attribute the extreme difficulty experienced in cutting it with short bladed scarificators.

Elastic contraction.—This is the second phenomenon of elasticity. It is the narrowing of the orifice of coarctations which have been enlarged by dilatation. It reproduces strictures which have been treated by that method. This property of the new tissue is produced in two manners. The first

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consists in an abrupt shortening, in virtue of which the orifice of the obstacle just enlarged contracts and loses instantly a part of the length given it by dilatation. This is the *abrupt* elastic contraction. The second manner of contraction is *slow, insensible*, and commences where the other ceases, reducing the orifice of the coarctation to the diameter it had before dilatation was practiced.

Abrupt, elastic contraction has been observed by all practitioners, and it cannot be attributed to spasmodic contraction of the canal or turgescence of the spongy tissue. It is very variable in extent and intensity, being greater in old strictures with rigid walls than in recent. In some strictures of long standing, the orifice of the coarctation returns instantly, after dilatation, to nearly its former size. In recent strictures, as an average, the orifice loses one-half of the diameter given it by sounds. I have remarked that this abrupt contraction was not so great in the commencement of dilatation as it is when this treatment has been pushed very far.

Among the cases of stricture, where this property was marked, I cite the following in the hospital de la Pitié, under the care of M. Michon. The stricture was situated in the pénienne portion of the urethra. The patient had been treated four times by the method of dilatation, and though sounds from six to seven millim. could be passed, the jet of urine was very small.

At my suggestion, M. Michon resorted to the method of exploration described by me, and he discovered, by means of a flexible bougie à boule, that the orifice of the stricture was as narrow after dilatation as before. How can this instantaneous narrowing be accounted for, if it is not owing to the elastic contraction of the heteromorphous tissue? To be certain that the difficult micturition was not caused by prostatic engorgement, nor by a valvule at the neck of the bladder, I examined the prostate by the rectum, and measured it by means of a catheter. This patient, at first thought cured by dilatation, was afterwards operated on by my method of urethrotomy by M. Michon, and with success.

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I have often demonstrated à l'hôpital Beaujon, in presence of M. Robert, the elastic contraction of certain strictures. That surgeon also experimented after my method of exploring. He carried the dilatation beyond the normal calibre of the urethra, to see if he could destroy this elastic property. To effect this, he used a dilator, its end rounded as an olive, and covered by vulcanized caoutchouc, the whole being about two centim. in diameter. The walls of the obstacle were widely separated by this instrument, but the next day the contraction could be discovered by a bougie à boule five millim.

The daily use of the sound proves the existence of this abrupt elastic contraction. Every surgeon using a bougie with a ball at its extremity, finds it as difficult to withdraw the instrument, after it has passed the constriction, as it was to introduce it. So soon as the enlargement of the bougie passes the narrowed portion of the canal, the tissue contracts around the neck of the instrument, and holds it fast.

Slow, insensible, elastic contraction is that quality which overcomes slowly the dilatation obtained by sounds—that degree of enlargement which persists for a time, after withdrawing the sound. This must not be confounded with the contraction of cicatrices by atrophy. Whatever may be the analogy in their results, the two kinds of contraction are not from the same cause. The first, that which we are speaking of, is too quick to be produced by the habitual contraction of inodular tissues. We sometimes see the enlargement obtained by sounds disappear in two or three days, but in the larger number of instances the dilatation remains for fifteen days or a month. This differs much from the interstitial or atrophic contraction, which requires some years to diminish sensibly the calibre of the canal. I believe this insensible contraction continues until the tissue of the stricture regains the size it had before dilatation.

Coarctations, with rigid and resisting walls, lose the degree of dilatation given them more promptly than those with supple and extensible walls, where the abrupt, elastic con-

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traction is less active, and the dilatation can be more considerable.

The elasticity of the tissue of stricture is now incontestible, and the study of its effects leads to important consequences:

1. The extensibility of the tissue explains why the orifice of stricture is enlarged with such facility.

2. The elastic contraction, abrupt or insensible, accounts for the narrowing of the orifice and reproduction of organic stricture. From this it follows, that the method of dilatation cannot be considered curative; that sounds, in dilating the tissue, are as incapable of destroying its elasticity as they are of producing by compression molecular resolution.

Elasticity is inherent in the tissue of coarctations; the constant return of strictures treated by dilatation is a proof of it.

The return of stricture after dilatation is too quick to ascribe the relapse to the contraction by atrophy. To admit that the relapse is due to this cause, one should think that as much time would be required for the reproduction of stricture cured by dilatation, as was required for its formation at first—that is ten or fifteen years. Such is not the fact. We know that the lesion requires only a few months to return to its former calibre. This we can be certain of by using the bougies à boules after dilatation has been discontinued for fifteen days or a month.

3. Incisions alone can paralyze the elasticity of the tissue, and urethrotomy is applicable not only to strictures with solid, resisting walls, but to those, though dilatable, which oppose the free passage of urine.

From my experiments and observations, I think I may say, as a general conclusion, it is impossible to restore to the urethra, by dilatation, its normal calibre when it is obstructed by an organic stricture. The orifice of the coarctation retains ordinarily, and that for a short time, only half the diameter given it by instruments of dilatation, whatever may be the length of treatment and volume of the sounds

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employed. This statement may be objected to, I know. To say that dilatation does not give to the orifice of stricture, even momentarily, a diameter equal to that of the sound which traverses it, appears a denial of what we see, especially when the patient can pass urine in a full stream, and the largest sounds are introduced. But the sounds have a special action on the tissue in making feeble its resistance and rigidity, or augmenting its suppleness and extensibility, so that the excretion of urine and introduction of sounds are facilitated. The tissue, as a spring long fatigued, becomes feeble, offers less resistance, though it does not lose its inherent elasticity.

ART. IV.—*Mortality of Powhatan County for the year* 1854. Collated from the Returns of the Commissioner of the Revenue. By POWHATAN S. DANCE, M. D.

IN collecting the following facts, as reported by our commissioner of the revenue, I do not intend to arrange them, in order that I may endeavor therefrom to deduce any general laws, as we are not warranted in so doing, where there are only a few isolated facts and cases upon which our laws can be based. But I only wish to test the efficacy and accuracy of the law in regard to the duties of the commissioner of the revenue, as it respects the registration of births and deaths, and if possible, to propose some remedy for its defects.

In the act of the general assembly of Virginia, chap. 25, § 8, the law states that the commissioner shall ascertain the number of births; also how many are born alive, and how many "still born." Which facts he is to gather from the heads of families in which the births take place. In the 9th section of the same chapter, the commissioner is required to ascertain the cause of all deaths, "if practicable," and to register them in a book kept for the purpose.

It will at once be perceived, that the results thus obtained

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must be very doubtful and indefinite. For example, in the case of births occurring among slaves, the commissioner gets his information from the master perhaps 10 or 11 months after the child is born; and should the child be then alive, he will scarcely recollect the time of the birth, (unless he keeps a family record, which all masters do not do.) If the child should have come dead, or have died soon after its birth, it is more probable that he does not remember the time of such birth or death; whereas the doctor's daily account book will show the time of such births as well as the condition of the child.

In regard to deaths generally, the objections to the law are more obvious, for in these cases the cause of death is to be specified, as well as the time of its occurrence, and the age of the patient. As it respects the causes of death, they are not unfrequently so obscure as to elude the searching and scientific investigation of physicians; and yet should the disease be accurately ascertained by the doctor, it perhaps is so mystified, and rendered meaningless to the uninitiated by its technicalities, as to escape the memory. As to the time of the death, the same obstacles to accuracy occur; whereas as in the cases of births, 10 or 11 months may elapse between the time of the death and the taking of the list, and consequently the master cannot speak positively as to the time. There is still another barrier to impair the value of these lists for the purposes of medical statistics—that is the age of the patient at the time of his death. Now, this is a point upon which masters cannot be expected to be accurately informed, as slaves so frequently change hands; and moreover, they have no special interest in knowing; and yet it is of great interest to the physician; for without the knowledge of this fact, he could not proportion his medicine; and he can generally ascertain the age of his patient either from himself, or some member of the family, and in no case should it be neglected.

In the above suppositions, I have designedly taken the births and deaths as occurring among the slave population,

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because they are not as indelibly impressed upon the minds of the masters as would be the births and deaths occurring among the white members of their family. I have no doubt but that one object of the law in question was to accumulate data and medical statistics from which general laws might be deduced; but the following facts will prove how inaccurate would be the premises upon which you have to proceed. Thus, of 156 deaths that occurred in this county in 1854, the cause of 33 is unknown, according to the commissioner's report. Again—as to the time of the deaths, there is a great want of precision. For example, of the 156 deaths, the time of 31 is entirely unknown—and then there is a vague and imperfect recollection of the time and cause of others.

But for the want of these important facts, and the accuracy that should characterize medical statistics, I do not mean to reproach our commissioner, for I am sincere in my belief when I say that there is no officer who is more competent for the performance of his duty, or who is more faithful in the execution of it, than he—nor do I mean to insinuate that the heads of families are careless and negligent in respect to their duty. I conceive that the fault is in the law itself.

By a reference to the law, it will be seen that the list is given in by the heads of families when the commissioner makes his annual official visit; which may be, as we have said before, 10 or 11 months after the birth or death of the person, and he has probably forgotten the time of the birth, or the cause and time of the death, together with the person's age. Or, as is frequently the case, the list is given at public places on public occasions, when the list will be more defective still.

Now, with all due diffidence, I would propose a plan whereby the commissioner would be exempted from a great deal of trouble, whilst at the same time the statistics would be infinitely more accurate and of more importance to the medical profession. It is this, that each physician be fur-

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nished with a number of printed blanks, analogous to those furnished ministers for making returns of marriages, in which he shall record the death of each patient, according to the form given in the blank. It would be comparatively of little trouble; for if the 156 deaths were divided equally among the physicians of this county, it would only give each one eight deaths to record in 1854—that is, presuming each physician to get an equal share, which I admit is not so. At any rate, the task would be a light one, and the record itself would prove an invaluable book of reference, and thus we might have established an accurate table of statistics, by which we could compare the relative health of the city and country at different times—the immunity from disease which the slaves enjoy, as compared with the whites, together with the influences which age, sex, color, occupation, &c. &c. exert on each race.

For the purpose of arriving at the ratio of mortality in Powhatan county, I shall calculate the population of this county, as ascertained by the last census, which is the latest information to be obtained, as the property book of the clerk makes no mention of slaves under 12 years of age, they not being taxable.

	Black males.	Black females.	White males.	White females.	TOTAL.
	2782	2500	1229	1284	7795
Deaths in 1854,	59	51	26	20	156
Proportion.	1 in 74	1 in 49	1 in 47	1 in 64	1 in 52

We perceive that the rate of mortality was nearly the same for all except the white females, which is one in 64. I know of nothing to attribute this to, except the fact that they are not so much exposed as the slaves and white males. The total number of deaths among the slaves is 110, making one death in every 48. The total number among the whites

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was 46, which is one in 54. The number of deaths among the males, including black and white, was 85—making one in 47. The number among the females was 71—making one in 53.

I am presuming now that the population is the same that it was in 1850, when the last census was taken.

Together with these condensed statements, I send you the entire tables, arranged as I thought most convenient, not with the hope of extracting therefrom any new law or theory, but only as an incentive to the profession, to urge its followers to labor more zealously to unfold those hidden treasures which might so much enrich the annals of medicine. You will find from the tables sent you, that one-twelfth of the deaths were from consumption, one-twelfth from typhoid fever, one-thirteenth from dysentery, one-seventeenth from pneumonia, and one-twenty-second from old age.

☞ See tables on pages 217 and 218.

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Number of Deaths, together with their Causes, as far as known, for December 1853 *and January and February* 1854.

CAUSE.	Black males.	Black females.	White males.	White females.	AGE.	CAUSE.	Black males.	Black females.	White males.	White females.	AGE.
					Y M D						Y. M. D.
Old age,	1	-	-	-	75 00 00	Pneumonia,	1	-	-	-	55 00 00
Child bed,	-	1	-	-	36 00 00	Dropsy,	-	-	1	-	55 00 00
Unknown,	-	1	-	-	00 00 07	White swelling,	-	-	1	-	22 00 00
Whoop'g cough,	-	-	1	-	00 02 00	Rheumatism,	-	-	-	1	85 00 00
Consumption,	-	1	-	-	35 00 00	Dysentery,	-	-	-	1	64 00 00
Typhoid fever,	-	1	-	-	45 00 00	Dropsy h'd & h't,	-	-	1	-	62 00 00
Pneumonia,	-	1	-	-	16 00 00	Convulsion,	-	-	-	1	00 01 00
						Total No. 14.	2	5	4	3	

Number of Deaths, together with their Causes, as far as known, for March, April and May 1854.

CAUSE.	Black males.	Black females.	White males.	White females.	AGE.	CAUSE.	Black males.	Black females.	White males.	White females.	AGE.
					Y M D.						Y. M. D.
Pneumonia,	-	1	-	-	18 00 00	Unknown,	1	-	-	-	25 00 00
Unknown,	-	1	-	-	59 00 00	Pneumonia,	-	1	-	-	25 00 00
Do.	-	1	-	-	19 00 00	Unknown.	-	-	1	-	67 00 00
Cancer,	-	1	-	-	50 00 00	Consumption,	-	1	-	-	40 00 00
Unknown,	1	-	-	-	00 04 00	Unknown,	1	-	-	-	75 00 00
Con tipation of bowels,	-	-	-	1	22 00 00	Pneumonia,	-	1	-	-	36 00 00
Whoop'g cough,	-	-	-	1	01 00 00	Asthma,	1	-	-	-	45 00 00
Typhoid fever,	-	-	1	-	72 00 00	Unknown,	1	-	-	-	00 06 00
Consumption,	-	-	-	1	20 00 00	Paralysis,	-	-	1	-	70 00 00
Pneumonia,	1	-	-	-	60 00 00	Unknown,	-	1	-	-	32 00 00
Unknown,	-	-	1	-	75 00 00	Do.	1	-	-	-	21 00 00
Consumption,	-	-	-	1	32 00 00	Colic,	-	-	1	-	08 00 00
Old age,	-	1	-	-	80 00 00	Typhoid fever,	-	1	-	-	08 00 00
Consumption,	-	1	-	-	20 00 00	Do.	1	-	-	-	30 00 00
Do.	-	1	-	-	22 00 00	Dysentery,	1	-	-	-	05 00 00
Asthma,	-	-	-	1	84 00 00	Pleurisy,	1	-	-	-	60 00 00
Pneumonia,	1	-	-	-	02 00 00	Pneumonia,	1	-	-	-	21 00 00
Teething,	-	1	-	-	00 05 00	Croup,	1	-	-	-	01 00 00
Do.	-	1	-	-	00 04 00	Unknown,	-	1	-	-	00 01 00
						Consumption,	-	-	-	1	68 00 00
						Total No 39.	13	15	5	6	

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Number of Deaths, together with their Causes, as far as known, for June, July and August 1854.

CAUSE.	Black males.	Black females.	White males.	White females.	AGE.	CAUSE.	Black males.	Black females.	White males.	White females.	AGE.
					Y. M. D.						Y. M. D.
Dropsy,	1	-	-	-	47 00 00	Paralysis,	-	-	-	1	79 00 00
Smothered,	-	1	-	-	00 04 00	Croup.	-	1	-	-	01 00 00
Unknown,	1	-	-	-	02 00 00	Dysentery,	-	-	-	1	14 00 00
Do.	-	1	-	-	70 00 00	Do.	-	-	1	-	12 00 00
Dysentery,	-	-	1	-	06 00 00	Do.	-	-	-	1	05 00 00
Do.	-	-	1	-	01 00 00	Do.	-	-	-	1	02 00 00
Do.	-	-	-	1	56 00 00	Do.	-	-	-	1	01 06 00
Do.	-	-	1	-	19 00 00	Cancer,	-	-	-	1	56 00 00
Do.	-	-	-	1	11 00 00	Worms,	1	-	-	-	08 00 00
Do.	-	-	1	-	01 06 00	Consumption,	-	-	1	-	66 00 00
Unknown,	1	-	-	-	75 00 00	Typhoid fever,	-	1	-	-	12 00 00
Typhoid fever,	-	-	-	1	72 00 00	Do.	1	-	-	-	01 06 00
Smothered,	-	1	-	-	00 06 00	Dysentery,	-	-	1	-	04 00 00
Dysentery,	-	-	1	-	01 00 00	Do.	-	-	1	-	02 00 00
Consumption,	-	-	-	1	38 00 00	Do.	-	1	-	-	03 00 00
Do.	-	-	1	-	30 00 00	Do.	-	-	1	-	04 00 00
Dysentery,	1	-	-	-	00 05 00	Old age,	1	-	-	-	80 00 00
Scald,	-	1	-	-	02 00 00	Cholera,	-	-	-	1	17 00 00
Unknown,	-	1	-	-	55 00 00	Dysentery,	1	-	-	-	00 05 00
Convulsions,	-	1	-	-	00 00 06	Unknown,	-	1	-	-	14 00 00
Disease of head,	1	-	-	-	00 05 00	Brain fever,	-	1	-	-	01 06 00
Typhoid fever,	-	-	-	1	33 00 00	Unknown,	1	-	-	-	70 00 00
Pneumonia,	1	-	-	-	55 00 00	Do,	-	-	1	-	14 00 00
Dysentery,	1	-	-	-	20 00 00	Burn,	-	1	-	-	02 00 00
Scrofula,	1	-	-	-	21 00 00	Old age,	1	-	-	-	80 00 00
Violence,	1	-	-	-	25 00 00	Inflam. brain,	-	-	1	-	01 02 00
Brain fever,	1	-	-	-	00 08 00						
						Total No. 53.	16	12	13	12	

Number of Deaths, together with their Causes, as far as known, for September, October and November 1854.

CAUSE.	Black males.	Black females.	White males.	White females.	AGE.	CAUSE.	Black males.	Black females.	White males.	White females.	AGE.
					Y. M. D.						Y. M. D.
Spasm,	-	-	-	1	00 00 11	Consumption,	-	-	-	1	55 00 00
Unknown,	-	-	1	-	00 01 00	Burn,	1	-	-	-	50 00 00
Typhoid fever,	1	-	-	-	22 00 00	Consumption,	-	-	1	-	40 00 00
Whoop'g cough,	1	-	-	-	00 09 00	Smothered,	1	-	-	-	00 00 14
Old age,	1	-		-	80 00 00	Unknown,	1	-	-	-	00 06 00
Apoplexy,	-	-	1	-	55 00 00	Do.	1	-	-	-	01 06 00
Congestive fev'r,	-	1	-	-	00 06 00	Do.	1	-	-	-	01 06 00
Unknown,	-	1	-	-	07 00 00	Liver disease,	-	-	1	-	35 00 00
Consumption,	1	-	-	-	30 00 00	Derangement,	-	1	-	-	40 00 00
Old age,	1	-	-	-	75 00 00						
						Total No. 19.	10	3	4	2	

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To complete these statistics, we have yet to add *thirty-one* deaths, not included in the foregoing tables, because the commissioner could not ascertain the time at which the deaths occurred. Of the 31, 18 were *black* males, 13 *black* females. Of these, 4 died of typhoid fever, 3 of old age, 2 of colic, 2 of consumption, 1 of pneumonia, 1 of dysentery, 1 of paralysis, 1 of mumps, 1 of the sting of an insect, 1 of worms, 1 of dropsy, 1 from violence, 3 smothered, and 9 from "causes unknown."

ART. V.—*Thoughts on a Visit to the Exposition Universelle in Paris, November 1st*, 1855. By DAVID P. HOLTON, M. D.

EVERY department of labor has within a few years received great modifications, which are destined to yet greater extension.

This recognized fact was recently very strongly impressed in examining the articles in the department of surgical instruments at the palace of industry.

We do not now propose to give an enumeration of new instruments, or the important changes in the fabrication of ancient ones, or their new application on principles long recognized.

Let us confine our view to one item, apparently the smallest—the simple *seton.*

A few years past, the vulcanized caoutchouc, of American invention, has been generally used for the seton.

This has recently been modified in the practice of M. Chassaignac, surgeon in chief of the new and model hospital, Lariboisiere.

Setons of vulcanized caoutchouc, as modified by M. Chassaignac, are hollow cylinders of variable diameters, perforated at fixed distances.

This tube is found to secure all the usual advantages of setons, recognized since ancient times, while it creates less irritation, and better harmonizes with the restorative powers of the system.

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Thus, without the introduction of principles really new, this system of drainage truly changes the aspect of general surgical practice.

In multitudes of cases, where the lancet or bistourie has heretofore been used, we now see applied, with less disadvantage, this vulcanized caoutchouc seton. . . . . These tubes are employed in various abscesses, purulent deposits, white swellings, diffuse phlegmonous swellings, some varieties of panaris, infiltrations of the scrotum from urinary fistula, arthrites and other inflammations. The catalogue of cases might be greatly extended.

Let us state a single case, as witnessed at the Lariboisière, October 29th, 1855. An encysted tumor of the breast. The skin is not discolored, and is not adherent to the movable tumor, from which it is separated by a thickness of tissues. Indistinct fluctuation is predicated. It is a case indicating the passage of the seton through the focus of the disease, which is done as follows:

The breast, including the tumor, is suitably held. Two points are selected, between which is found the centre of the tumor. At the first point is made to enter the trocar with its canula to the centre of the tumor, when the trocar is temporarily withdrawn, allowing any fluid to flow out through the canula, with a view to test the course made and the contents presented. This delay also reassures the surgeon as to the proper direction of the remaining half of the canal to be made.

The trocar is returned through the canula, yet in its place, and both are passed onward and outward through the skin at the second point above named.

Then the trocar is finally withdrawn, and through the canula is made to enter a wire, a waxed thread or a small bougie, having at its opposite extremity the attached seton, which is drawn through the canula, so that its two extremities shall be equally distant from the centre of the tumor.

Finally, the canula is withdrawn, leaving the seton—the perforated hollow cylinder of vulcanized caoutchouc—in the position proposed.

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Judging a second seton requisite, the surgeon in like manner introduced it, penetrating the tumor in a direction different from the former.

In sundry other cases in the hospitals of Paris, we see the number of setons is yet more increased, and this system of drainage, by virtue of its observed benefits, is generalized to a degree which gives a new and improved aspect to surgical practice.

In the whole world of medical and surgical practice the greatest modifications and ameliorations are referable to the discovery of anæsthetic agents.

Whatever may be their number and mode of administration, my heart always beats with gratitude for the national honor which must ever attach to the United States for the *practical initiative* of this great discovery—this greatest ameliorator of surgery.

It is hoped that congress will yet more munificently reward the discoverers, or their families.

The labors and experiments of Drs. Wells, Jackson and Morton were severally known to their individual friends, but the melancholy death of Dr. Wells has left his personal merits and claims in this discovery to be eclipsed.

The interests of science and the cause of justice require a more thorough examination of their individual share of credit.

From America etherization was popularized in England by Drs. Ferguson, Key, Guthrie, Liston and others.

From England the discovery rapidly passed, by the press and by private letters, into France, and was communicated to the Academy Jan. 12th, 1847, by M. Malgaigne.

About the time that *ether* was thus generally introduced, there was made an extended research for other agents securing similar effects. Nor was this multiplied experimentation in a way already opened in the new world, destitute of results, and England is soon to erect a monument to commemorate the discovery of chloroform by Dr. Simpson.

Will the great republic do less in honor of those who had

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antecedently discovered this wonderful property of ether, and applied it to assuage the sufferings of humanity?

Will not the American medical association organize a subscription to which every citizen may be permitted to add one dollar each for the erection of a monument to commemorate the initiative in *anæsthetic agents*, taken by their countrymen?

LECTURES AND SELECTIONS.

I. *Observations on Hæmorrhages arising from Implantation of the Placenta upon the Cervix of the Uterus; with Remarks on the Pathogeny and Therapeutics of this Accident.* By M. Legroux, Physician to Hôtel-Dieu of Paris, Agrégé Professor to the Faculty, etc. etc. (Archives Générales de Médecine, Dec. 1855.)

Hæmorrhage consequent upon the insertion of the placenta over the cervix of the womb, has, latterly, been the subject of much discussion, both in learned societies and medical periodicals.

The most complete and important document which has appeared among recent publications is Dr. Depaul's report* to the Academy of Medicine on a case of complicated labor, communicated by Dr. Gérard.

More recently, Professor Dubois has taken up this matter in his clinical course; and his lectures, which first appeared in the Journal de Médecine et de Chirurgie Pratique, have been very generally republished.

After such imposing authorities in obstetrical matters have given their opinions, I almost fear to enter on the question, or to give the results of my personal experience. For, if I have observed and appreciated facts aright, the generally admitted doctrine in respect to the pathogeny of placenta prævia must be laid aside. If I am not deceived, some of the precepts of the masters of obstetrical art must be modified or revised, and a new therapeutical element must find a

* Bulletin de l'Académie, July 1852.

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place in their teachings. May the desire and hope of being useful to humanity justify the boldness of my undertaking!

In the first place, let me briefly recall the reigning doctrine on the pathogeny of this hæmorrhage.

"It is generally admitted that the flooding becomes more profuse as labor advances, and the separation of the placenta, from which the hæmorrhage arises, becomes more considerable; that whatever increases the uterine contractions necessarily augments the bleeding; that the means of arresting this are precisely those which suspend the contractions, for the hæmorrhage lessens and ceases only in the intervals between the pains." (Gardien, 2nd ed. t. ii, p. 404.)

And farther on, (p. 406,) it is stated that "the flooding produced by the separation of the after-birth, at any other part of the internal surface of the womb, augments when the true labor pains are suspended, and ceases when the pains are active."

The reason of this difference is that, in the latter case, the womb closes the orifices of the uterine vessels, as it contracts; whereas, in the former, the contractions of the body and fundus dilate the cervix, and separate the placental attachments more and more, the distended vessels remaining open-mouthed.

According to this theory, the hæmorrhage is *placental.* A multitude of objections are at hand, but I pass them by for the present. With the opinion of Gardien, I collate those of Désormeaux and Professor Dubois.

"Blood flows more abundantly during uterine contraction in hæmorrhage from insertion of the placenta over the cervix, while, in other hæmorrhages, the compression of the womb suspends the flooding. The blood, in the former case, is forced out of the vessels of the cervix and of the placental parenchyma by the recession of the circumference of the uterine orifice, and by the pressure of the child on the placenta." (Dict. de Méd. art. Metrorrhagie, t. xix, p. 660.)

This doctrine, attributed to Duparcque, admitted by Désormeaux, M. Depaul, and Professor Dubois, and uncontradicted, so far as I know, is very similar to that of Gardien; only the latter attributes the hæmorrhage to *the gradual separation of the placenta, the vessels of which remain gaping;* whereas the other theory takes account of the pressure of the fœtus, and assigns an utero-placental origin to the hæmorrhage. In both theories, however, the uterine contractions are considered the primary cause.

The hæmorrhage is in direct relation to the *activity* of the labor, and the hæmostasis depends on its *suspension.*

The practical deduction is this: *to arrest the hæmorrhage, moderate the uterine contractions.*

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But, inasmuch as parturition must go on, after all, we are advised, in the face of theory, to hurry the labor by ergot, in order to hasten the delivery of the placenta. But we are necessarily placed between two perils. If we excite uterine contraction, as practical sense would seem to dictate, we incur the hazard of profuse and fatal flooding. If we moderate the pains, according to the indications of theory, the blood does not flow so fast, but the patient almost invariably succumbs to the progressive anæmia.

I hope to substitute for this theory *one which subordinates the hæmostasis to uterine action and reciprocally*, and which will permit uterine action to be augmented without danger.

I shall state, in the first place, the facts on which this theory is based. My first cases* are designed to determine the source of the hæmorrhage.

CASE I.—A woman, æt. 30, had been flowing, at intervals, for a fortnight, when she was brought to the Beaujon hospital in April 1852.

She was near her term. She had lost much blood; the fœtal movements had ceased. She was still flowing moderately; and though of robust habit, was pale and feeble. The os uteri admitted the end of a finger, and presented no abnormal appearance. (*Rest in horizontal posture, astringent drinks and injections.*)

The hæmorrhage ceased, but returned in a few days; the patient became faint. I asked the advice of Dr. Robert, surgeon to the hospital. By digital and speculum examination, we made sure of a prominence of the posterior lip; the finger, after passing the os, perceived a spongy substance. The woman bore the examination, in both the horizontal and vertical positions, without discomfort.

We decided to give ergot. The medication had not been commenced, however, when, towards the close of the day, the woman, without more flooding, died unexpectedly in a swoon.

Autopsy.—Central implantation of the placenta over the os uteri. Placenta soft, brown, decomposed, not fœtid, however; placenta easily detached. Fœtus decomposed. No visceral lesion to explain the sudden death.

The decomposition of the placenta, resulting from the death of the fœtus, forbids us to locate the hæmorrhage in that organ. If the bleeding was utero-placental at the outset, it was assuredly purely uterine after the child's death.

Notice also a fact, which is constantly observed in cases of placenta prævia, that flooding occurred long before labor, and therefore independently of uterine contraction, to which theory assigns the chief share in its production.

* We have found it necessary to abridge the reports of cases, but have endeavored to omit no important fact. Otherwise, we could not have published M. Legroux's paper in a single number, as it occupies over thirty pages of the Archives, a greater space than we can afford.—*Translator's note.*

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The following is a succinct summary of a case observed in 1844, at the hospital Saint-Antoine, and recorded with great care and detail by my friend Dr. Bernutz, who has kindly communicated it to me.

Case. II.—A woman, æt. 44, was in the eighth month of her thirteenth pregnancy. She menstruated the first four months, but had seen nothing since, until towards the end of the eighth month, when, after bad treatment from her husband, she began to flow, and lost considerable blood at intervals for three days. The flooding ceased after a protracted syncope, and the next day the woman was brought to the hospital.

She was pale, but there was no hæmorrhage. The os was dilated to the size of a quarter of a dollar, and was filled with a substance which felt like *cup moss.*

In the night, copious hæmorrhage, sighing respiration, epigastric pain, feeble, frequent pulse, *no uterine contraction*, same state of os. (*Cold vaginal injections.*) The hæmorrhage ceased. The patient took a little wine, and felt better. The tampon was employed, but the patient complained of it, and of a great pain at the epigastrium; the stomach retained two scruples of ergot, which had been given in three doses. Soon after, the patient became delirious, the pulse failed, and a state of syncope ensued, which resisted every mode of stimulation.

Autopsy.—Heart contains black and fibrinous coagula; in the great vessels a quantity of blood analogous to what is usually found in examinations *post-mortem.* The uterus contains a fœtus at term in a state of decomposition. The placenta, implanted on the anterior surface of the cervix, completely closes the orifice, and rises a few lines beyond the posterior border. The os is dilated to nearly the size of an half dollar piece, and is filled by the tampon, which is hardly stained. No open vascular orifices.

The anatomical conditions of the placenta are not mentioned in this case. Notwithstanding this omission, we may assume that the utero-placental circulation was more or less completely intercepted after the death of the fœtus; for, according to Professor Moreau, when the fœtal circulation ceases, the blood coagulates in the uterine vessels, and many of the latter are obliterated. Only enough blood goes to the womb as may suffice for its nutrition; the *stimulus* which invited more is gone, and hence dilatation may occur without much hæmorrhage, albeit the vessels which unite the placental borders are torn. This is Dr. Cazeaux's rational explanation of the dilatation of the cervix without hæmorrhage.

Death, in the two cases I have cited, was not the direct and immediate, but the secondary, effect of the hæmorrhage. In the second case, indeed, the fatal termination was brought about by a series of accidents; but in the first, it occurred unexpectedly, and without extreme exhaustion.

I think it useful to dwell on the dangers of the anæmic

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state induced by repeated floodings, dangers which continually menace the patient, even after a protracted cessation of the bleeding. I shall insist, also, on the danger of some of the obstetrical manœuvres which may be requisite under these circumstances.

CASE III.—A year ago, a woman was brought, at the hour of my visit, to my 'ying-in ward at Hotel-Dieu, who had been several days in labor, and was exsanguined by repeated hæmorrhages.

A spongy substance, that was evidently the placenta, filled the orifice of the womb, which was detailed more than two inches, (6 *centim.*) The case was urgent. The extreme debility of the patient indicated that she could not withstand another hæmorrhage. I attempted to deliver. Contrary to my anticipations, the cervix was so unyielding that I could not dilate it by introducing my fingers. I had scarcely commenced my manœuvre when the woman swooned and seemed about to expire. I desisted, and ordered stimulants, broths, sinapisms, and left directions that the tampon should be used on the slightest recurrence of hæmorrhage.

Hæmorrhage did not recur, but the patient sank gradually, and died in a few hours.

Autopsy.—Placenta over cervix, as had been recognized during life. No clot between the uterine and placental walls to explain the hæmostasis. It was impossible to discover, at points where the placenta was separated, the orifices of any vessels.

What else could have been done for this poor woman, in her state of extreme exhaustion? Artificial delivery would obviously have caused instant death, since even the introduction of the hand into the vagina induced syncope. Should the membranes and placenta have been perforated? There were no uterine contractions, and the sudden removal of pressure from the abdominal contents, after the escape of the waters, would have aggravated the syncope. I have since regretted that I did not use the tampon, which might have induced uterine contraction, and stimulated the vital powers.

In this case, also, death took place many hours after hæmorrhage had ceased, and was a secondary result.

I observed the same thing, lately, in the case of a lady of Courbevoie, whom I attended with two eminent colleagues. She was exhausted by repeated hæmorrhages occurring in a lingering labor. When I saw her the flooding had stopped. The os was largely dilated; a large, spongy mass was behind it; the tissues were all relaxed. Extraction of the placenta, version and extraction of the child seemed easy. We decided that this operation was indicated. I practiced it. The whole manœuvre required less than a minute. The lady complained, at the moment of extraction, of faintness and a sensation of anguish. No blood followed. Notwithstanding, and de-

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spite the devoted care and attention she received, this patient succumbed in a few hours, without reacting.

In the last six months, two cases of artificial delivery in placenta prævia have been in my lying-in ward, under the care of Dr. Danyau,* specially charged with cases of dystocia. Both of them succumbed two or three days after the operation.

The simple extraction of the placenta, after the child is born, produces the sensation of anguish I have alluded to, in a woman exhausted from hæmorrhage. One morning during my visit, I found a young woman blanched by hæmorrhage, which doubtless was due to inertia of the womb after expulsion of the child. The extraction of the after-birth seemed urgently requisite. I relied on the contractions which the introduction of the hand would excite; but this simple operation, as rapid as it was easy, produced, at the moment the placenta came away, a dying sensation, which was followed, in a few instants, by actual death. Death was certainly impending, and inevitable perhaps, in the exsanguined state in which I found this woman; but I asked myself if I had not accelerated the fatal event, by performing an operation.

From these facts, the number of which I could readily increase, two conclusions may be drawn: 1. That anæmia due to repeated hæmorrhages, an anæmia which I would term *acute*, in contradistinction to the chronic anæmia induced by unfavorable hygienic conditions, which may be extreme without endangering life—that acute anæmia, I say, exposes patients to sudden and unexpected death, and demands all the solicitude of the practitioner. 2. That the slightest obstetrical manœuvre, during the debility of acute anæmia, may superinduce a fatal perturbation.

After this digression, which I have thought might not be devoid of practical interest, I return to the facts which illustrate my views of hæmorrhage in placenta prævia.

Case iv.—Mrs. Lemasson, æt. 28, rue St. Antoine, reached the eighth month of her eighth pregnancy in May 1847, at which date she had an hæmorrhage which seemed to threaten a premature labor.

Moderate venæsection, indicated by a somewhat plethoric habit, and cold applications to the abdomen and thighs checked the bleeding. It returned, however, on three occasions, before term, and was thrice con-

* Dr. A. C. Danyau, son of an accoucheur formerly in great vogue in Paris, and son in law of old Roux, is surgeon to the Maternité. He is one of the eight members of the section of accouchements at the academy, and is great authority at the Surgical Society on matters pertaining to obstetrics. He has been in practice twenty-six years.

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trolled with the same facility; but the third time, the bleeding was not restrained entirely, and it became abundant after three or four days, when labor began.

The presumption of placenta prævia was confirmed, as soon as dilatation was sufficient to admit the finger, which encountered a spongy mass, so thick as to conceal the prominences of the fœtus. The dilatation reached about two inches and then ceased, notwithstanding the intensity and frequency of the pains.

On exploring the orifice, carrying the finger as high as possible, I passed, on the left, between the internal surface of the cervix and the placenta, and ascertained the following facts:

During the uterine diastole, the finger passed readily between the separated parts; but then the blood streamed by it into the vagina.

During the systole, the finger was driven out by the tense membranes, which were pressed firmly against the uterine walls; *the blood then ceased to flow, but that which had been poured into the vagina during the diastole was expelled by the downward pressure of the womb.*

Having repeatedly satisfied myself of the correctness of these facts, I considered it evident that the issue of blood from the vessels, the *hæmorrhagic fact*, was diastolic, though the appearance of the blood externally, the *apparent hæmorrhage*, was systolic indeed, and coincided with the suspension of the real hæmorrhage.

The hæmostasis was manifestly the consequence of the uterine contraction, of the firm application of the membranes on the inner surface of the cervix.

In order to arrest the hæmorrhage, it was necessary to maintain this state of things during the repose of the womb; to cause the combined mass of the waters and the fœtus to press on the cervix in the intervals of the pains. The vertical posture fulfilled this indication.

I boldly substituted it for the horizontal position hitherto adopted. The patient, too feeble to stand alone, was held by strong persons on either side, while I sat in front, supporting her feet and knees by mine, and sustaining her with one hand, while I manœuvred with the other. As soon as she was placed upright, there came a torrent of liquid blood and coagula well calculated to frighten me and cause me to repent of my experiment. But my fears were speedily dissipated; the first gush over, *the hæmorrhage ceased entirely!*

I satisfied myself by the touch that, in the intervals between the pains, the womb remained on the floor of the pelvis, and that the weight of the waters and fœtus maintained the separated vascular surfaces in apposition.

The uterine contractions became at once stronger and more frequent. But the os did not dilate. What was the obstacle to dilatation? Undoubtedly, the peripheric adhesions of the placenta, detached on a small part of the left side only.

I attempted, passing my finger up on the left, to reach the membranes, but unsuccessfully. I presently detected, however, a fissure between the placental lobules which I separated, tearing with the finger nail the parts that offered resistance. I finally reached the membranes, and ruptured them during a contraction. A large rent was immediately torn in the placenta as the waters escaped. The placenta was thrust to the right by the head, which engaged at once, and after delivery was accomplished without any accident. *The child was living and lusty.*

In a portion of its circumference, the placenta was torn for about two and a half inches. Near this rent was a smooth white surface, the cicatrix, probably, of an antecedent laceration.

The woman had a good getting up.

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Case v.—A few months after this confinement, Mrs. Lemasson became pregnant again. The abdomen increased rapidly in volume. By the eighth month it was enormous.

During this month, she suffered from several hæmorrhages, each of which lasted several days, and was treated by rest, refrigerants, astringents and injections. Before her confinement, the bleeding became continuous, and produced a state of anæmia. The pains were feeble; they were not aroused by ergot; the os did not dilate.

The feebleness was such that I could not resort to the vertical posture. I plugged the vagina with bits of lint saturated with a solution of alum. When the tampon was removed, it was evident that it had suppressed the hæmorrhage completely.

The presence of the tampon excited contractions. In three hours, I cleared out the vagina, and found notable dilatation of the orifice, which was occupied by a spongy mass. Following the periphery of the orifice, I found, on one side, a fluctuating membranous protrusion, which I ruptured. The head engaged and rapidly enlarged the opening. In a very few minutes two children were born. They had been dead for some days. The after-birth was delivered without difficulty.

The subsequent history is less pleasing. The patient narrowly escaped death from metro-peritonitis. She had double *phlegmasia dolens*, followed by incomplete anchylosis of the knees, which was not ultimately relieved, until she had passed two seasons at the Bourbonne springs.

The menses have not returned since; but at the supposed monthly periods, the patient suffers from hypogastric pains and hysteriform symptoms requiring anti-spasmodics, and occasionally, leeches to the anus. The general health is blooming, however, and these periodical indispositions are less severe at each return.

This complete observation furnishes a demonstration of the propositions I have advanced, as will appear, I hope, from the following considerations:

Source of the hæmorrhage.—When the child is dead, and the placenta decomposed, it is obvious that the bleeding is purely uterine. This was the case in the first patient whose history has been detailed.

Admitting that after the death of the child, the placenta remains grafted to the walls of the womb, from which it receives materials for its nutrition through special vessels, it is evident that the vascular connections between these organs must be too restricted, the utero-fœtal circulation having stopped, to produce a notable hæmorrhage, such as caused death in case 2.

In one case as in the other, the bleeding was exclusively, or almost exclusively, uterine.

Is it so when the circulation between the mother and child is intact?

A priori it might be argued that, nothing being changed in the progress of the accident, the blood comes from the same source. This rational deduction receives a practical confir-

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mation from the case of Mrs. Lemasson. In her case, the bleeding was arrested by maintaining the placental and uterine surfaces in contact, while a portion of the placenta was exposed over an orifice two inches in diameter. If this organ had been the source of the hæmorrhage, would it not have bled freely, while thus exposed, during the active pains excited by the vertical posture?

Therefore, in all cases, the hæmorrhage is almost exclusively uterine.

The death of the child, which precedes delivery in the majority of cases, (in seven out of eight that have been under my care,) would allow us to suppose that the hæmorrhage, at the onset, was placental and fœtal. But there was no placental hæmorrhage in Mrs. Lemasson's case, and her child was living and not anæmic by any means. The death of the fœtus is due to the interruption of the utero-placental communications.

It may be objected that Dr. Simpson's plan of extracting the placenta at once arrests the hæmorrhage: *Sublata causa, tollitur effectus.*

If this objection was valid, the decomposition of the placenta should have the same effect, which it has not. Why this difference? In the latter case, the placenta, although deprived of vitality, still retains its mechanical connections with the uterine walls. As these connections are broken up by the retraction of the walls of the womb, vascular orifices are exposed. Hence those intermittent hæmorrhages, which occur during labor, although the child has ceased to live. In Dr. Simpson's operation, the portion of the uterine surface corresponding to the placenta is exposed at once; uterine contraction is excited by operative manœuvres, and the vascular orifices are closed. Hence the cessation of the hæmorrhage.

It is unquestionable that this operation would be injurious in cases of inertia of the womb.

The practical deduction from this theory is: *hasten, expedite the labor.*

The suspension of hæmorrhage is commonly attributed to the formation of an obstructing coagulum. I have never been able to find this coagulum in any of my autopsies. If the hæmostasis was due to such a salutary clot, the therapeutical deduction would be: let the clot alone, retard the labor, a most pernicious result.

In the exposure of the uterine sinuses by the growth of the womb during pregnancy, and by the dilatation of the os during labor, in this mechanism, we find an explanation

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of the phases of apparition, suspension, and recurrence of hæmorrhages from placenta prævia.

Nature of the hæmorrhage.—Dr. W. Mackenzie's experiments on bitches near the full term of gestation, equally tend to prove that in partial separation of the placenta, bleeding occurs from the denuded uterus, and not from the separated placenta. From these same experiments, and from injections into the uterine vessels, through the hypogastric arteries, of defibrinated blood, this English author concludes that inter-utero-placental hæmorrhage is *arterial.* With Dr. Jacquemier* I am unwilling to admit this conclusion, when I consider the prodigious development of the uterine venous apparatus during gestation, and the free communication between all parts of it. This question, however, has more theoretical than practical importance. The capital point is to determine what organ furnishes the blood.

Relation of the hæmorrhages to uterine contractions.—I repeat my remark that hæmorrhage in placenta prævia occurs long before term oftentimes, and is consequently independent of uterine contractions.

It has been asserted that the hæmorrhage, in placenta prævia, takes place at each pain, and is arrested in the interval, and that the reverse obtains when the placenta is separated from its attachment to the fundus or body.

In the first case, the observation is correct, but the interpretation is erroneous. I have shown that the placenta is, at the most, only a very secondary source of hæmorrhage. Common sense suggests the improbability of much effusion of blood between the surfaces of the ovum and uterus, when these surfaces are strongly pressed together; and what common sense indicates, experience corroborates. It is, indeed, a matter of ordinary observation that, in the horizontal posture, the flooding ceases in the interval, and returns with the pains; but the interpretation of this fact is that the *real hæmorrhage* occurs in the interval, and that the *apparent hæmorrhage* is only the elimination of blood already effused.

In the second case, observation has certainly been defective, but the conclusion is correct. The hæmorrhage does occur during the repose of the womb, but it is impossible to believe that the flooding is manifested externally at this period; it is only when the womb contracts that the blood that has filled the vagina during the diastole is expelled.

It is incorrect, therefore, to institute any distinction be-

* Gazette hebdomadaire de Méd. et de Chir. March 24th, 1854.

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tween the different insertions of the placenta, in respect to the time at which the hæmorrhage takes place.

Vertical position in labor.—In the horizontal position, in which the parturient woman is commonly placed, the uterine contractions force the womb towards the vulva and the fœtal extremity towards the cervix. When the pain is over, the womb reascends in the pelvis, and the child falls back in the uterine cavity. The weight of the ovum is supported by the posterior wall of the uterus; a part of the force of each contraction is expended in raising the fœtal mass, and depressing the womb to the point at which the preceding pain left it; this force is lost as far as expulsion is concerned.

In ordinary labor, the womb is equal to its double task; but when the pains languish, the force of the contractions is barely sufficient to raise the fœtal mass and imperfectly depress the womb. The labor becomes stationary, and may remain so for hours or days, unless art intervenes.

Ergot may render good service in such cases, or it may be insufficient, and the forceps may be resorted to. Now, under these circumstances, the vertical posture alone, or aided by a dose of ergot, will almost invariably induce sufficiently active labor to bring about a natural delivery.

In this position the fœtal mass presses constantly on the cervix, and is a permanent cause of dilatation. However feeble the contractions may be, they are exclusively employed in expulsion, for the womb ascends but little in the intervals. Moreover, this position almost always excites more frequent and energetic pains.

By this means, the use of the forceps may be avoided* in a great majority of cases, a desirable result, inasmuch as many persons who practice obstetrics are not familiar with the application of these instruments.

I have already treated of the hæmostatic effects of this position. It assuredly does not always succeed, however, and may often be prohibited by the exhaustion of the patient. What resource have we then?

The tampon.—Methodical plugging of the vagina, advocated in 1776, by Leroux of Dijon, is now generally admitted to be an invaluable remedy.†

* I was present, a short time since, when a distinguished obstetrician delivered a robust lady by means of the forceps. She had been in labor three days in the horizontal posture. While the operator was preparing the bed, the patient experienced a desire to go to stool, and was placed for a moment on a chair *ad hoc*; such violent pains came on, that if the operation had been delayed for a few minutes, the child would inevitably have been precipitated into the vessel.—*Author's note.*

† Dr. Legroux introduces here a long extract from a lecture of Professor Dubois on the tampon, which we omit, as we intend to publish that lecture entire.—*Translator's note.*

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I usually introduce the speculum, and fill the vagina with little balls of linen or charpie by means of a long pair of dressing forceps, and when the tamponnement is complete, I apply a T bandage. Many accoucheurs employ the simple pessary of vulcanized india-rubber, invented by Gariel, and recommended particularly by Dr. Chailly. This is inflated after its introduction; the facility with which it is applied permits frequent examinations during the labor, without discomfort to the patient.

Perforation of the membranes.—To evacuate the waters, and allow the uterus to contract and close the vascular orifices laid bare by the separation of the placenta, is a practice advised by the majority of authorities, in all cases of profuse flooding towards the end of gestation.* Nearly all, however, require that the commencement of labor should be clearly indicated by unmistakable pains.

The author of the excellent treatise from which I learn these facts, considers rupture of the membranes preferable to the tampon. Professor Dubois is so convinced of the utility of rupturing the membranes, that he lays it down as a maxim that this manœuvre should be employed in preference to forced delivery by the podalic version; but he makes it necessary that there should be *strong and frequent pains, a certain degree of dilatation and dilatability of the cervix, and a favorable presentation.* This is something more than a "commencement of labor," certainly. It reduces us to the ordinary indications for the operation almost.

Dr. Cazeaux (*loc. cit.*) observes that, after hæmorrhage, the pains are generally feeble, and that labor may advance considerably without any noticeable pains; that the pains may be suspended after having been strong and frequent, although the cervix is dilated and soft. In these two cases, the condition of the cervix will determine the accoucheur's conduct. If this is favorable, the best way to excite or renew the contractions will be to rupture the membranes.

But, notwithstanding the authority of Dr. Cazeaux, I cannot believe it prudent to perforate "when the end of the finger can be introduced, and feels the membranes growing tense at intervals." I am more struck by the dangers than by the advantages of perforation at this stage. It is not certain that this operation will arrest hæmorrhage. If delivery is not rapidly accomplished, the contractions will separate other parts of the placenta, and the hæmorrhage will return. A premature evacuation of the waters retards

* Cazeaux, Traité des Accouchements, p. 772. Paris, 1850.

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the labor, and far from being favorable to the child, subjects it to the dangers of uterine compression. When the tampon affords this security, why not wait until the os is sufficiently dilated and soft? Moreover, if hæmorrhage returns after premature evacuation of the waters, we cannot have recourse to the tampon with the same confidence; the blood may accumulate in the uterus.

When the placenta is partially implanted on the cervix, a period always arrives when a portion of the membranes protrudes at the os. It is at this point, obviously, that they should be ruptured at the opportune moment. But when the placenta is centrically implanted, should *it* be directly perforated? Dr. Gendrin adopted this procedure in two cases. He traversed the centre of the placenta with a female catheter; the waters escaped, and the hæmorrhage ceased. Notwithstanding his success, I should not dare to imitate him. If such a perforation is converted into a laceration, and the fœtal head passes through it, the placenta may be caught upon the shoulders, and constitute a grave obstacle to delivery. I deem it safer to wait for dilatation, and to rupture as near the edge of the placenta as possible. The escape of waters will make a rent, through which the fœtal extremity will pass, pushing the placenta to one side.

The proposal to rupture the membranes early is attributed to Prezos. But it is to be observed that this accoucheur, by dilating the os with his fingers, in the first place, excited contractions, and that active pains being induced, the membranes could be ruptured with less hazard.

It is admitted that, when the waters are evacuated, the contraction of the womb brings the fœtus in contact with the vascular orifices, whence a salutary compression. Now it is not certain that contraction will ensue; neither is it certain that some prominence of the fœtal ellipse will be pressed against the part at which the placenta is separated. Do we not deprive ourselves of the most efficacious compressor, of an upper tampon or bladder pessary, by evacuating the waters prematurely? Before disgorging the ovum, is it not rational to wait until the fœtus, closely compressed on all sides, will suffice for a tampon?

To sum up what I have said, the conduct of the accoucheur in the floodings towards the end of gestation, due to separation of the placenta, is as follows:

1. To use the tampon, in the form, if possible, of the bladder pessary, which allows the condition of the parts to be examined, without discommoding the patient.

2. To place the patient in a vertical position, having her

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held if necessary. This position completes the tamponnement. It suffices alone, if the labor is active.

3. Induce and promote pains by ergot, if requisite.

4. Perforate the membranes when the pains are strong and regular, when the cervix is soft and dilated, when the presentation is good.

5. If the tampon and vertical posture fail, the membranes must be ruptured prematurely, which will temporarily restrain the hæmorrhage.

6. Lastly, if the hæmorrhage still persists, and menaces life, the placenta must be extracted.

Extraction of the placenta.—Complete separation and extraction of the placenta, advised by Dr. Simpson of Edinburgh, will arrest the hæmorrhage immediately, at least in the majority of cases. This operation is indicated when the means I have described are unavailing. It is an extreme resource. It is inadmissible unless the child is dead or not viable, and delivery by version or the forceps is impossible. (Dubois.)

Case.—A woman, at term, was seized with an hæmorrhage, which was attributed to insertion of the placenta on the inferior segment of the uterus. Unsuccessful efforts were made to perforate the placenta; but, even after tearing away a portion of the placental parenchyma, the operator failed to reach the amniotic cavity. The tampon did not arrest the bleeding.

The woman was carried to the hospital *des Cliniques*. The membranes were ruptured, and ergot was administered. The uterus contracted incompletely; the hæmorrhage moderated, but was still alarming.

Professor Dubois presently arrived, and found the vagina filled with blood. The os was dilated to the size of a silver dollar; the placenta was implanted over it centrically. The pulsations of the fœtal heart could not be perceived.

As the want of dilatation forbad an attempt to deliver by version or the forceps, M. Dubois observed that it was legitimate to separate and extract the placenta. He introduced his hand, secured an edge of the placenta, separated, twisted, and extracted it. The hæmorrhage ceased. In an half hour, active pains came on, and the child was delivered in five hours.*

This case proves that I was correct in supposing the rupture of the membranes was not a certain hæmostatic. Inertia of the womb might allow the hæmorrhage to continue after the placenta was extracted, also. There cannot be complete security, therefore, after that operation.

Artificial and forced delivery.—If the means that have been described did not arrest hæmorrhage, rather than stand by helplessly while life was ebbing, it would be right to resort

* Journal de Médecine et de Chirurgie Pratique, June 1855.

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to an hazardous operation, to deliver forcibly, making an incision in the cervix if necessary. Such an operation will be very rarely indicated.

But when there is good dilatation, and a soft and yielding cervix, ought we to deliver immediately by version or the forceps?

I may be wrong; but, in such a case, I should wish to wait for natural delivery, if I was master of the hæmorrhage. I should be still more inclined to this course if the waters were intact, and I could count on the amniotic bladder as an upper tampon to aid my vaginal or lower tampon. If the waters have escaped and the woman is exhausted, artificial delivery is the only resource. But, I repeat, I have a dread of the perturbation produced by obstetrical manœuvres at such a moment. If the woman is so feeble that artificial delivery is deemed too hazardous, she should be placed with her head low, her limbs raised by pillows; a bandage should be placed around the abdomen; hot wine, broth, and other restoratives should be administered; sinapisms should be placed on various portions of the skin. Perhaps these means will revive the powers, and the labor may be accomplished.

I will not dwell longer on these great obstetrical difficulties. My experience does not warrant me in saying more.

Let me, in conclusion, adduce a single etiological remark.

All the cases of placenta prævia of which I have learned the history, occurred in women who had borne several children, in whom the uterine cavity was necessarily larger than in primiparæ, a condition favorable to the descent of the fecundated ovum into the lower part of that cavity. Prophylaxis is at fault in such circumstances, unless it might be prudent for a woman predisposed by an antecedent placenta prævia, to pass the early part of a subsequent pregnancy in an horizontal posture.

II. *Lectures on Varicella, Variola, and Variolous Diseases.* By Armand Trousseau, Professor of Clinical Medicine to the Faculty of Paris, at Hotel-Dieu, &c. &c. [Translated from l'Union Médicale.]

Lecture II.—Varioloid.

To explain my opinions concerning varioloid, I must be permitted to borrow an illustration from a well known fact in natural history.

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A farmer sows choice grain, of wheat, for example, and for two, three, four, or five years, he reserves from the crops sprung from this original seed, grain enough to plant in the ensuing season. But, by the end of the fourth or fifth year, he finds, from the price that his miller offers him, that his grain has degenerated. Yet it has been sown each year in equally fertile soil, and has been cultivated with equal care. The farmer understands that he must go to the next market town and purchase what agriculturalists term *seed wheat*, if he wishes that his next crop should be of the best quality. This degeneration of seeds is a matter of daily observation.

Observation teaches us, likewise, that if the same seed is planted in different localities, varying in temperature, in altitude, in the nature of the soil, and in degree of humidity, prodigious modifications result. If you study botany in a flat country, you find a tolerable uniformity in vegetation; but if you herborize among the mountains, as you ascend from the alluvial valleys to the snow-capped summits, you are almost astounded by the progressive degradation of vegetable species. You find the *myosotis*, for example, a foot high in the valley, but dwarfed to less than an inch near the summit. You recognize the plant by its flower, but how different are all its other external characters. If you take the seed of this dwarf myosotis of the mountain top, and plant it in a genial soil, after successive generations, it recovers its primary attributes.

This degeneration obtains in animal species also. Animals that have a large stature and a rich fur in one country, lose both size and the color or texture of their coat under other skies. Look at the innumerable variety of dogs, at the different breeds of horses, cows, etc. in England, where the art of raising cattle is so well understood, that they have at will, fat pigs and sheep, milch cows or beef for the table, racers or powerful draught horses.

These preliminary considerations on the degeneration of vegetable and animal species will introduce us to the modifications of morbid species, which, when introduced into modified constitutions, likewise undergo various degenerations. We shall understand how certain morbid species do not fructify in certain soils, so to speak, just as animal and vegetable copulations are not always, thank heaven! fruitful. Yet the sperm of the male has each time touched the female organ, whence we necessarily conclude that either the capacity of the female or the quality of the male sperm is imperfect. Some seeds will not germinate in certain soils. It is so with animal germs also. It is so likewise with morbific

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germs. It is not surprising, then, that the economy should be so modified as not to receive and mature certain morbid germs, or develop them in an unusual manner.

Experience demonstrates that when a child has once had a certain neurosis, whooping cough, he becomes incapable of receiving the morbid germ which produces whooping cough. The change which his organism has undergone is not appreciable by the eye of the anatomist or physiologist, but it does not escape the physician's eye. Enlightened by experience, the physician declares that the organism of the child is so modified that he will not have whooping cough again. The same thing obtains in measles, scarlatina and variola. We all know that a person who has had small-pox, repels a future assault of this disease nine hundred and ninety-nine times in a thousand. What has taken place in his economy? We do not know, any more than we know why a girl cannot conceive before the age of puberty, or why a woman cannot be impregnated after a certain age.

This inaptitude to receive morbific germs is not always absolute; it may be relative. We still find a comparison in vegetable physiology. A thousand seeds thrown in a thousand different soils, will produce plants varying in height, in foliage, sometimes in inflorescence even. So the same morbific cause, implanted in a thousand different individuals, will be developed into morbid varieties.

An individual who has had variola or vaccinia, may lose, after a time, that precious organic modification which gives him immunity, and may become liable to receive the morbific germ of variola a second time. But he will receive it after a peculiar fashion, and it will be modified as it is developed. Suppose a certain number of persons who have acquired the modification we speak of. Those in whom it occurred a year or two ago will repel the variolous miasm almost invincibly; those who were subjected to it ten years ago will, some of them, be affected, but in a peculiar manner; those who were subjected to it twenty years ago, will be affected in a different manner, and so on; whence so many varieties in the expression of the morbid symptoms arising from the same poison? This principle gives us the key to the numerous varieties of varioloid.

Varioloid is nothing but variola developed in persons who have not entirely lost the liability to variola. This liability or aptitude is lost by antecedent vaccination or small-pox. Varioloid is a variola that a man takes who has lost a greater or less part of the immunity he acquired by vaccination or small-pox. In these conditions, a man subjected to the

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variolous virus will have a disease which will recall its origin more or less. There result varioloids in which you can scarcely detect the traces of variola, and varioloids which you can hardly distinguish from legitimate cases of small-pox. Between these two extremes, there are a multitude of intermediate shades of difference.

Varioloid, then, is only modified small-pox. This is proved by the simplest and clearest experiments. When a person with varioloid comes in contact with unprotected individuals, he communicates legitimate and unmistakable small-pox. This is a strict demonstration. It is the same seed, surely, for, finding a virgin soil, it attains its original development.

The forms of eruption vary in varioloid. Hence ten or fifteen varieties, which are not readily defined. We shall attempt a general sketch of the disease, and briefly indicate its varieties.

Initial fever.—In variola, the initial fever usually commences with violent headache, vomiting, pain in the back and loins, general lameness, accompanied, in the adult, with sweats, and, in children, with diarrhœa and convulsions. In confluent small-pox, this fever lasts forty-eight hours; on the third day the eruption appears. In the discrete form of the disease, the fever commonly lasts seventy-two hours, the eruption appearing on the fourth day of the malady. In both cases, the eruption comes out in twenty-four or thirty-six hours; at the close of which period all the papules which are to compose the eruption are visible.

The initial fever of varioloid, according to observations made, not in thousands, but in hundreds of thousands of cases in Germany, France and England, differs but very little from the initial fever of true variola. There is one phenomenon, however, which is quite common in the incipient stage of varioloid, which is peculiar. This is an eruption resembling that of scarlatina, appearing, on the second day of the disease, on the neck, inner part of the forearm, groin, and sometimes on the abdomen. Occasionally, this eruption presents a violent tinge, as in some grave cases of variola accompanied by *purpura*, but the cases of varioloid do not present greater gravity on this account. On the whole, there is very little difference in the duration and intensity of the initial fevers of variola and varioloid.

Eruption.—In discrete small-pox, the eruption appears, as I have said, after the lapse of seventy-two hours. On the second day of the eruption, the pimple is slightly raised; the prominence is greater on the third day, but the circum-

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ference of the pimple is also increased, and it consequently appears to be somewhat flattened. On the fourth day, the eighth of the disease, the pustule fills with lactescent serum, enlarges, and becomes umbilicated. At the same time, an inflammatory areola appears, which, in discrete variola, is quite large, and produces tumefaction. Each areola is from a sixth to a third of an inch broad. It attains its greatest size on the tenth day of the disease; at the end of the tenth day, it begins to diminish, and disappears completely on the eleventh or twelfth day. The pustule then resembles simply a white drop, like a drop of congealed wax. On the eleventh or twelfth day, the pustule breaks and discharges, and there remains only a scab, which falls off from the twentieth to the twenty-fifth day. This is the course of things in discrete variola; and it is this form of small-pox we must always have in view when we compare the disease with varioloid. The pustules are commonly of the same size on the same parts of the body, but in different parts their bulk may be different. They are usually large on the forearms and hands; on the face they are smaller than on the trunk and limbs. Those on the hands, for example, are very unlike those on the face. From the fifteenth to the eighteenth day, large umbilicated blebs of purulent serum may be seen on the hands; whereas, by the ninth or tenth day of the disease, the small pustules of the face begin to desiccate, and, by the thirteenth day are completely dried up.

In varioloid, the progress of the eruption is very different. The first day, the eruption is like that of variola; on the second day, the pimples are pointed; on the third day, they are still more prominent; on the fourth day, they fill with lactescent serum, and may, or may not, present a slight umbilication; but they are never large enough to resemble drops of wax. In variola, if you pass your hand over the surface on the fourth day of the eruption, you perceive slight inequalities which yield softly to pressure. In varioloid, on the contrary, if, on the fourth or fifth day, you pass your hand gently over the patient's surface, closing your eyes in order to attend more closely to your impressions, instead of this soft sensation, you notice a rough inequality, a surface strewn with warty tubercles. On the ninth day of the eruption, all the pustules are hard, horny, and dried without suppuration. On the eighth day of discrete variola, a febrile movement supervenes, denominated *fever of maturation*. Unless the disease is very intense, this fever is usually absent in varioloid. Therefore: 1st, modification of pustules, which dry up without suppuration after the fourth

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or fifth day; 2d, absence of the fever of maturation on the eighth day. Such are the fundamental characteristics which distinguish varioloid from variola.

But these features are not always so strikingly manifest. When variola or varioloid is inoculated, with extreme prudence, of course, on a person who has lost the immunity he derived from vaccinia almost entirely, the varioloid that results is a legitimate small-pox almost.

In the epidemics of England and Germany, varioloid has been observed closely resembling frank small-pox. Hence, on the verge of the total loss of the immunity acquired by vaccination, variola reassumes the major portion of its characteristics, still preserving some traces of varioloid. Look at the patient in the St. Agnes ward. That man was vaccinated ten years ago. He now has varioloid. The onset of his disease indicated a severe attack of small-pox. The eruption appeared, at first, precisely like that of the most intense variola. But soon the lingering influence of vaccinia happily showed its power. Instead of the great tumefaction of the face which attends a confluent eruption, there was only a moderate swelling; instead of profuse salivation, there was only slight ptyalism; there was no delirium, none of those cerebral symptoms which menace the lives of small-pox patients. Look at the patient's forehead; see the pustules of different sizes, like those of variola, (I do not speak of confluent variola, in which innumerable bullæ cover the face with a gray mask, that mask of *Joseph-paper*, according to Bailly's striking expression—I speak of discrete small-pox only;) see, I say, those irregular angular pustules on our patient's forehead. If we had attended only to these, we should have thought we had to do with a case of small-pox. But the characteristics already mentioned undeceived us. In our patient, suppurative fever set in on the eighth day, but, contrary to what occurs in small-pox, it diminished on the ninth day, and totally disappeared on the tenth.

In demi-confluent or confluent variola, the tumefaction of the face as it ceases, should be followed on the tenth, or, at the latest, on the twelfth day, by swelling of the hands or feet, without which the patient inevitably dies; it is Sydenham who says so, and daily observation corroborates his rule. In our case, notwithstanding the confluence of the eruption, the hands and feet have not swollen, and we are now at the thirteenth day. Instead of finding the pustules of the arms and hands enlarging, so that they may resemble, on the fourteenth day, drops of melted white wax that have cooled, and on the fourteenth to the seventeenth day may break and

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discharge, as happens in small-pox, we notice, in our patient, that almost all the pustules of the arms are already horny. Of a hundred pocks, there are only five or six that are suppurating, and this suppuration is unlike that of discrete variola.

Thus, in this confluent eruption, which seemed to appertain to a grave case of variola, the progress of symptoms has taught us, by a series of striking characteristics, that it was modified small-pox or varioloid that we had to deal with. These characteristics were: the slight tumefaction of the face, the slight salivation, the absence of delirium, the absence of swelling of the feet and hands, the desiccation and induration of the pustules without suppuration. As a general rule, desiccation commences on the fifth, sixth or seventh day of the eruption, in varioloid. In the case we have adverted to, desiccation began on the eighth day. It was not until the eighth day then, in this instance, that the characteristics of varioloid were plainly manifest; on the tenth day, they were unmistakable.

I have here delineated varioloid on the verge, as it were, of variola. Between such cases and those in which the disease displays scarcely any resemblance to small-pox, there are an infinity of intermediate shades. It is important that you should remember that, in varioloid, the eruption is so discrete, sometimes, that there are only ten or fifteen pustules on the whole body; that, in other cases, there may be a near approach to a confluent eruption. Recollect also, that as long as an individual retains the slightest trace of the immunity acquired by vaccination, the influence of vaccinia will be made manifest, either in the general or local symptomatic expression of the malady. Of this you have a striking example in the man in the St. Agnes ward.

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CHRONICLE OF MEDICAL SCIENCE.

MATERIA MEDICA AND PHARMACY.

1. *On Carbazotic Acid.*

The great number of new compounds which organic chemistry brings to light continually, furnish inexhaustible objects of enquiry to that industrious class in our profession which conceives that disease is to be overcome by multiplying our weapons for combatting it. Among the substances of this character lately brought into notice, is carbazotic acid, which was described by Professor Calvert at the meeting of the British Association last year. We find some account of it, and of its therapeutical properties, in the Association Medical Journal for August 1855.

It is formed by the action of nitric acid on indigo, aloes, etc. and appears as shining yellow scales, soluble in water, to which it gives an intensely bitter taste. It consists, according to Liebig, of 15 atoms of carbon, 3 of nitrogen, and 15 of oxygen. It is soluble in alcohol and ether, and unites with salifiable bases. The carbazotates of ammonia, potassa, etc. have been used in medicine.

Dr. Bell of Manchester is said to have cured intermittents with this acid. (Braithwaite, Jan. 1856, p. 41.) Dr. Moffat has employed it in several diseases, (Assoc. Jour. p. 742,) and reports that it is a good tonic. He says that it tinges the skin yellow, and produces an artificial jaundice which it is difficult to discriminate.

2. *Cod-Liver Oil Oleine.*

Dr. Arthur Leared, the author of "a theory of my own of the digestion of the fat," which has not received the assent of the physiological world, is quite positive that it is the oleine of cod-liver oil that is available for nutrition, and that the other principles are excrementitious. Our readers will remember that Dr. Crawcour considers *glycerin* the potent element in this drug, while De Jongh attributes great virtue to the *gaduine*. However, *chacun à son gout*. Dr. Leared's

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plan for extracting oleine is to submit the oil to a very low temperature, and after it has congealed, to subject the semi-solid mass to pressure. Seventy-five per cent. is obtained of a very transparent fluid, which is universally tolerated, says Dr. Leared, by the weakest stomachs, and produces all the good effects of cod-liver oil, and none of the bad effects. Dr. Leared adduces cases observed at the Brompton hospital in support of these assertions, which are published in the Medical Times and Gazette for July 21st, 1855. In as much as the process is simple, we hope that some of our readers will repeat Dr. Leared's experiments, and inform us of their results.

3. *Ergot of Wheat.*

Dr. Jobert details in the Gazette des Hôpitaux the results of his observations on ergot obtained from wheat, which, as our readers know, is affected with this fungus, in common with the other *graminaciæ*. He states that its ecbolic and hæmostatic properties are equal and similar to those of ergot of rye. Repeated in doses of one or two scruples, according to urgency, it restrained hæmorrhages at different stages of gestation and after delivery, and promptly accelerated uterine pains.

4. *Syrup of Hydriodic Acid.*

Mr. Murdoch publishes in the Pharmaceutical Journal a method for the administration of hydriodic acid, recommended by Dr. Andrew Buchanan as a substitute for iodine, when the irritant action of the latter proves troublesome. We abridge Mr. Murdoch's directions, which we find quoted in the Dublin Hospital Gazette for October 1855.

Take of sugar ℥ ii ʒ ss; water ʒ v; dissolve in a flask with the aid of heat, and allow it to cool. Take of tartaric acid 264 grains; iodide of potassium, 330 grains. Dissolve each separately in ℥ iss of distilled water; mix the solutions, and strain through cotton cloth to separate the bitartrate of potash. Add water to make up the measure to ℥ vi¼. Each fluid drachm of this solution should contain 5.072 grains of hydriodic acid, equal to about five grains of iodine. Take ℥ ss ♁ i of this solution and add it to the syrup already prepared. The whole should measure ℥ iiss, and each fluid drachm should contain the equivalent of two grains of iodine. This syrup may be kept for several months unchanged.

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5. *On the Indigenous Substitutes for Quinine in Illinois.*

Dr. Goodwin of Illinois, in a letter to the editor of the Northwestern Medical and Surgical Journal, calls the attention of the profession to the various substitutes for quinine, which are the native growth of that country. We take the following extract from this paper:

"The hickory, (juglans) so common, is an excellent antiperiodic—the inner bark of either variety in decoction. Hickory tea can be recommended as a safe and effectual remedy in cases of simple and confirmed agues.

The wild crab apple, (pyrus malus) is another, growing so plentifully in the west. A decoction of the root forms a good remedy for chills.

The poplar (populus tremuloides) is a superior remedy, and can be much relied upon.

Dogwood (cornus sericea) is another powerful antidote, superseding by far the use of cinchona, or any of its preparations, in cases of long standing.

The branch willow, (salix alba) found along the margins of our creeks. The root or inner bark, in decoction, is an old and well known remedy in fevers of the south, and cures chronics more effectually than quinine. Its alkaloid (salicin) is being much used.

The chestnut, especially horse-chestnut, (æsculus lippocastanum) furnishes another remedy in the list.

Decoction of the corn blade and of corn meal will cure simple intermittents.

A decoction of hickory, willow and dogwood barks, one-half ounce each to a pint of water, taking a half glassful every two hours between the paroxysms, will cause them to succumb to its herculean powers! Or a decoction of any one single bark, one ounce to the pint of water, and dose one-half glassful, will fulfill its prescribed mission, that of curing as many, if not more, intermittents as cinchona or quinine."

6. *Cultivation of Liquorice in this Country.*

A correspondent of the New York Times (Mr. William R. Prince of Flushing, L. I.) is of the opinion that the officinal glycyrrhiza glabra may be easily cultivated in this country. He says: "The liquorice is one of the most important plants that is destined to be added to American agriculture, and merits at our hands an early adoption, on account of the

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facility of its culture, its great usefulness for various purposes, and for the large profit it yields to the cultivator. When the high priced lands of England are profitably devoted to it, how much more profitable must it prove, where land is plentiful and cheap, and where, above all, as in several of the western states, the soil is naturally permeable, free from all stones, and no manuring required. It is, indeed, mortifying to American pride, to witness the many thousands now paid to Europe for an article like this, so simple in its culture that we ought to be the largest exporters of it, thus adding another item to our 'granary of the world.'

"It has long been extensively cultivated in Spain, and from the commencement of Queen Elizabeth's reign it has been largely grown in various parts of England."

Besides its employment in medicine, liquorice is extensively used in the manufacture of porter and other preparations containing saccharine ingredients, and its introduction into this country could not fail to be profitable.

Boston Med. & Surg. Journal.

MEDICAL PATHOLOGY AND THERAPEUTICS.

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1. *Miasmatic and Contagious Diseases—the Distinction between them—and their Geographical Relations.*

The following are the conclusions arrived at by Dr. Mühry of Göttingen, in an interesting paper recently published on the above subject:

1. Miasmatic diseases are specially dependent upon the influences of soil, humidity, temperature and seasons, like a vegetation.

2. They generally manifest themselves and operate immediately after invasion, without any regular period of incubation—like a poison.

3. They may occur again and become chronic in the same individual, without any diminution of his susceptibility. (Receptivitat.)

4. They do not regenerate themselves in the human organism.

1. Contagious diseases manifest themselves quite independently of soil, and also (with some particular exceptions) of temperature and seasons.

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2. The attack always occurs after a longer or shorter, but regular period of incubation.

3. They generally occur only once in the same individual, and if they return it is after a long space of time.

4. They reproduce themselves solely in the human organism.

The relation of contagions to temperature, or the geographical distribution of such diseases may be thus expressed. The majority of them are ubiquitous, i. e. independent of temperature, and appearing in every zone of the world, irrespective of the period of the year. Examples of this truth are variola, scarlatina, rubeola, pertussis, cynanche parotidea, aphthae, puerperal metritis, erysipelas, hospital gangrene and malignant pustule. Those, on the other hand, which are influenced by temperature and the seasons, are divisible into three classes:

1. Those which are confined to high temperatures, and are common in the tropics—lepra, frambœsia, dysentery and aphthae.

2. Those which are most common in the temperate zones—erysipelas, puerperal metritis, pertussis and croup.

3. Those which are found in the regions between the tropical and polar zones—plague and typhus.—*Henle's Zeitsch. fur Rationelle Medicin.*

2. *Croup and Methods of its Treatment.*

Dr. Hönerkopff has recently published a paper, in which he extols the administration of the sulphate of copper in this disease. He has used this substance in 99 cases of croup, 77 of which recovered; and the total quantity administered by him to these patients was 2846 grains, or, on an average, 31½ grains each. He has never seen any poisonous effects result from its use, although one child got 27 grains daily for a week, or in all 216 grains; and another, 4½ years old, took 150 grains in 7 days, and a third, aged 2½, 120 grains in three days. No inflammation, gangrene, or other bad symptoms took place. In 8 out of the 13 cases which proved fatal, there were either other diseases coexisting, or the author was called to see them too late, so that he considers that his remedy failed in only 5 out of the remaining 82 cases. The mode in which he administered the salt was to dissolve from 6 to 8 grains in ℥j of water: and, according to the age of the patient, and the severity of the vomiting induced, to administer, more or less frequently, from a teaspoonful to a tablespoonful of this solution, until

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vomiting occurred. The nature of the vomiting should always regulate its administration; for the induction of vomiting is absolutely essential for the therapeutic action of the remedy, as it has a kind of specific influence on the disease.

Dr. J. Samter, of Posen, has also written a paper in praise of the sulphate of copper in this disease. His remarks on its advantages and mode of administration so closely resemble those of Hönerkopff which we have given, that it is unnecessary to quote them here at length. He uses a solution of from 4 to 8 grains, (and in severe cases, 10 to 12 grains,) in ℥ ij of water; of this he gives ʒ ij repeatedly, till vomiting is induced, and thereafter ʒ j every two hours.

In addition to the use of sulphate of copper, this author, at the beginning of the disease, applies four, eight, or twelve leeches to the larynx, and especially if there be any pain felt there, and he afterwards applies stimulating epithenes. He also uses the inhalation of powdered alum and sugar, employs warm baths, and envelopes the feet in hot sand, etc.—*Günsburg Zeitschrift*, vj, 2, 1855.

Dr. Luzsinsky considers that there are four therapeutical indications in the treatment of croup, which must be attended to by the physician. 1st. To change the peculiar blood crasis which exists. 2d. To prevent the localization of the inflammatory process in the larynx. 3d. To treat the spasm of the larynx. And 4th. To encounter and destroy the false membranes which have been formed.

For the fulfillment of the first indication, most men have recommended the use of mercurials, but Luzsinsky depends more upon alkalies, and especially on the carbonate of potass, which exercises a solvent action on all the albuminous products of the organism. Its use retards the development of the constituents of the blood, and greatly impairs the vital coagulability and inherent plasticity of that fluid. This is the theory of the action of this salt, which Eggert recommended as a specific in croup, after an experience of it in about 250 cases. It may be given advantageously in doses of from ʒ ss to ʒ ij daily. Carbonate of soda may do in mild cases, but the other alkali is alone to be relied on in more severe ones.

The second indication may be answered by the application of a blister, the size of a crown-piece, to the upper part of the manubrium of the sternum.

Spasm of the larynx is most surely treated by opium, applied externally, in conjunction with vesicants (15 grains to ʒ ss of opium, to ʒ ss of lard,) and also given in small doses internally.

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To arrest the formation of the pseudo-membranes, nitrate of silver, in a concentrated solution, may be applied to the fauces and entrance of the larynx. Emetics may thereafter be given, and they are only necessary in the exudative stage. Luzsinsky gives decided preference to the use of sulphate of copper, which he administers by giving, every 15 minutes, a teaspoonful of a solution of two or four grains (or even more,) of the salt in ℥ iiss of some fluid. He does not look upon tracheotomy, in croup, in a favorable light.

Out of 30 cases treated thus by Dr. L. only seven died.—*Oester. Zeitschrift*, i, 6, 8, 10, 1855; also *Schmidt's Jahrb.* xi, p. 207, 1855.

Dr. Menschel relates a case of croup in which, despite the timely and assiduous use of all the ordinary remedies for this disease, difficulty of respiration and danger of suffocation increased. He then painted the whole of the front of the neck very darkly with a strong tincture of iodine, (ʒ j of iodine to ℥ j of spirit,) and covered the part with flannel, smeared with digitalis ointment. After an hour the urgent symptoms had abated, the patient slept quietly during the night, and next day was convalescent.—*Preuss. Verein. Zeitung*, 10, 1855.—*Edinburgh Med. Journal.*

SURGICAL PATHOLOGY AND OPERATIONS.

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1. *On Chloroform in Military Surgery.*

We make an extract from a letter on this subject, by the eminent professor of military surgery, M. Baudens, published in the Gazette Médicale, Dec. 29, 1855.

"Many all important practical questions are still in a state of controversy: incisions in gun-shot wounds, extraction of splintered bones, the propriety of immediate or secondary amputations, the advantages of resection or amputation, etc. etc. Even the benefits of chloroform have been questioned, and, in the Sardinian army, they are afraid to use it. In the French army, on the contrary, this precious anæsthetic has been used in more than 25,000 cases. The physician-in-chief, M. Scrive, from whom I have a document attesting this fact, assures me that no accident whatever resulted from the use of anæsthesia in these cases.

"It is true that chloroform is used by our army surgeons

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prudently, according to the rules which I have proposed, and never transgress, intentionally, the stage of insensibility. This precept is based on the beautiful experiments of M. Flourens, which prove that the action of chloroform is progressive and successive as regards the nervous centres.

"A great advantage of chloroform is that it permits wounds to be dressed and put in order that are apparently inevitably mortal, and that the surgeon would otherwise let alone, fearing to increase the victim's sufferings. Wounds thus dressed will, at all events, give less pain, and unhoped for cures will sometimes result. Thus the portion of shell that I send with this letter weighing *over four pounds*, was extracted, in my presence, by surgeon-major Mercier from the muscles of the right thigh of the soldier Steven of the 57th, at the field-hospital before Sebastopol. The projectile was buried in the upper and outer part of the thigh; the femur was shattered; the shock was extreme. Yet, under the influence of chloroform, the fragment of shell was extracted, the limb was amputated, and the patient is now in a fair way to recover.

"Such facts speak louder than any criticism. It remained only for chloroform to be tested on the battle-field; it has triumphed completely."

2. *The Results of M. Sichel's Operations for Cataract.* By M. Doumic. (Annales d'Oculistique, tome xxxiv, p. 172.)

During the last nine years (1846–54,) M. Sichel has operated for cataract 1,026 times in 641 individuals. Taking the whole period, the two *sexes* furnished nearly the same number of patients, viz: 313 males and 328 females. The necessity, however, of dealing with a sufficient number of figures in determining their respective liability, is seen from the fact that while among the patients during 1846–51 (432) there was a preponderance of 44 females (238) over the males (194,) among those of 1851–4 (209,) the males (119) exceed the females (90) by 29. The *ages* at which the operations were performed, are thus set forth:

5 months to 10 years, 26; 10 years to 20 years, 19; 20 years to 30 years, 14; 30 years to 40 years, 13; 40 years to 50 years, 50; 50 years to 60 years, 136; 60 years to 70 years, 229; 70 years to 80 years, 139; 80 years to 90 years, 15—total 641.

In the first years of life, congenital cataracts are pretty frequent. Up to about the age of forty, the greater number operated upon are of congenital origin, the remainder being traumatic; spontaneous cataract occurring very rarely. After

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forty years, on the contrary, and especially after fifty, double senile spontaneous cataract becomes very frequent, the proportion of traumatic cataracts continuing the same.

The seat of these cataracts is stated to have been lenticular in 930; capsulo-lenticular 77; caspular 19—total 1,026.

The consistence of the cataract was hard in 23; semihard in 232; soft in 269; semisoft in 459; semiliquid in 42; ossified in 1—total 1,026. Thus the soft and semisoft cataracts, taken together, are of much more frequent occurrence than the hard and semihard united. Hence the reason for the more frequent performance of the operation of extraction; for in these cataracts the fragments, immersed in the aqueous humor, would swell, and give rise to various serious accidents, especially in persons liable to even slight chronic congestions of the brain. Moreover, the absorption of the *débris* of the crystalline takes place very slowly in the aged, when it takes place at all. It is therefore in these cases better to avoid any risk of compression of the internal membranes, and extract the cataract when it is soft or semisoft. In children, the fragments of the lens are absorbed rapidly after division, without accidents; and in them division of soft cataracts should be the rule. When the cataract is hard, depression should be performed at all ages, for the lens cannot swell, and it may be lodged in the lower and deeper part of the eye; and whether absorbed or not, will not there injure the internal membranes. But the nucleus may be hard, and the cortical substance soft, and then the cataract must be extracted if the cortical substance is abundant; while, when this is small in quantity, the cataract may be depressed without danger.

The results of the operations are given as follows:

Extraction, 780. Successful, 616; partial success, 71; failure, 93—total 780. Thus the proportion of successes is 79 per cent. as compared with 12 per cent. failures. In explanation of even this proportion of failures, it is to be observed that two of the nine years were cholera years, and vomiting impeded in some of the cases the healing of the corneal wound.

Division 136. Successful, 100; partial success, 24; failure, 12—total 136.

Depression, 98. Successful, 67; partial success, 19; failure 12—total 98.

The whole of the operations united gave the following results:

Cases.	Success.	Partial.	Failures.
1014	783	114	117
	73 per cent.	15 per cent.	12 per cent.

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The proportion of success attending the different operations was, in extraction, 79 per cent.; division, 73 per cent.; depression, 67 per cent.

3. *Vinous Cataplasms in Hospital Gangrene.* By M. Payan. (Gazette des Hôpitaux, No. 133.)

Hospital gangrene has frequently shown itself among the wounded sent from the Crimea to the hospitals in the south of France. It has been generally during the passage from Constantinople to Toulon or Marseilles that it has broken out; and it has resisted treatment in the hospitals with great obstinacy, usually requiring very painful cauterizations to arrest its progress. M. Payan has had under his care in the hospital at Aix ten cases, in different stages of the disease. Recollecting the great efficacy of vinous cataplasms in sanious ulcers, he determined to resort to them on this occasion, and found that a speedy amelioration ensued in all these cases upon employing them twice a day. Some slices of bread are placed in a pipkin and *vin ordinaire* is poured over them, and when the bread has thoroughly imbibed the wine, the whole is boiled for a few minutes, stirring it the while with a spatula, so as to form it into a kind of paste.

[We may observe that M. Denonvilliers has recently found glycerine of great service in these cases at the St. Louis hospital.]

OBSTETRICS, &c.

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1. *Statistics of Operative Midwifery in the Duchy of Nassau.* By Dr. Ricker of Ettville.

The memoir of Dr. Ricker is especially deserving of full analysis, because it exhibits an example of statistical research that fulfills most of the necessary conditions of accuracy and completeness. He gives a complete statistical account of the operative obstetrics for the whole Duchy of Nassau, for the years from 1821 to 1842 inclusive. The population of the Duchy is 429,341; there are a hundred private physicians, and about twenty more partly engaged as military or bath physicians. All are required every six months to make a return of their cases of obstetric operations. These are collected and systematically analysed.

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1. *Forceps Operations.*

After a close enumeration, it has been found that in the twenty-two years extending from 1821 to 1842, there were 304,150 births in Nassau.

Of these, 4,223 were terminated by the aid of the forceps—i. e. 1 in $72\frac{94}{423}$. It is, however, remarked, that in the earlier years the reports were less full than later, and that many operations remained unrecorded, so that 150 or 200 more forceps-deliveries ought to be added. This would give about 1 in 70.

Result of the forceps operations.—Out of 4,223, 93 women died either during or soon after the operation, and 684 children were still-born. Hence 1 woman out of $45\frac{38}{93}$ delivered by forceps, died; and 1 child out of $6\frac{412}{684}$.

2. *Turning.*

Turning by the head and *turning by the feet* are distinguished: Ten cases of head-turning are recorded, of which nine ended favorably to mother and child; in the tenth the child perished through morbid contraction of the pelvis. Dr. Ricker points to this as far happier than the result of foot-turning, and a strong incentive to further experiment.

Foot-turning was resorted to 2,473 times, or 1 in 123 times. The *results* of these 2,473 cases of turning were: 176 mothers died, and 1,431 children were either still-born or died shortly after birth. Thus 1 mother in $14\frac{9}{176}$, was lost; and for every $1\frac{142}{1431}$ child, one was lost.

3. *Perforation.*

One hundred and forty-three perforations were performed; that is, 1 to 2,165 labors. Of these, 88 mothers were saved, 35 died, and concerning 20 there is no certain information.

4. *Embryotomy.*

There were 22 cases of embryotomy; 16 mothers recovered, 6 died. This gives 1 embryotomy in 13,825 deliveries.

5. *Artificial Premature Deliveries.*

Only 3 artificial premature deliveries were effected: 1 by uterine frictions, 1 by a sponge placed in cervix, 1 by punc-

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ture of membranes: that is, 1 in 101,383 deliveries. All the mothers lived; 2 children survived, 1 was dead.

6. *Cæsarian Section in the Living.*

This operation was performed 12 times (of which in 1 the abdomen only was opened.) Two mothers and 7 children were saved; that is, 1 section in 26,000 deliveries. Contrasting this result with that of perforation or embryotomy, Dr. Ricker says, that since 9 lives—i. e. 2 mothers and 7 children—are saved, whilst in the latter cases all the children and one-third or one-fourth of the mothers perish, the Cæsarean section is rather more favorable.

7. *Cæsarean Section in the Dead.*

This operation was performed 27 times. The section of the abdomen only twice: 1 for abdominal gestation, 1 for rupture of the uterus. In 4 cases the dead pregnant woman was delivered in some other way, namely, once by forceps after death by convulsions, and three times by turning. In no one of these cases was a living child extracted.

From the returns it appears that 211,568 persons died from 1821 to 1842 inclusive. It thus appears that for 6,411 deaths and 9,436 births, there was *one* dead pregnant woman upon whom the Cæsarian section, or some other artificial delivery, had been completed.

Dr. Ricker concludes by observing, that it would be of great interest to procure like returns in other countries, in order to facilitate comparisons with those now recorded.

Condensed from the Med. Chir. Review.

2. *On the use of Potassio-Tartrate of Antimony, in the form of Enema, in Obstetric Practice.* By James Young, M. D. Methven.

In the interesting cases reported by Dr. Young in the Edinburgh Medical Journal for January 1856, we see displayed the relaxing influence of tartarized antimony in overcoming rigidity of the os uteri. Dr. Young thus succinctly records his observations on this subject:

In a primipara, æt. 31 and of full habit, the pains had been strong and frequent for six hours, the os uteri was thin in the edges, hard and unyielding; and the external parts contracted and tender. Sixteen ounces of blood were taken from the arm, eight grains of Dover's powder were administered, and fomentations applied to the external parts. Under these remedies the pains ceased for

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six hours. When they returned they affected the back, and were frequent and regular. After continuing for six hours longer the os uteri was still thin in the edges, hard, and not dilated more than the size of a shilling; the external parts narrow and contracted.

Ergot was inadmissible under such circumstances. Reasoning from analogy on the effects of tartar emetic as a local irritant, when employed in the form of lotion or ointment, I conceived that temporary local stimulation of the uterus might be induced more directly from the rectum than through the medium of the stomach, and that subsequent relaxation might be produced in the surrounding parts, without the production of nausea and general prostration. One grain of tartar emetic dissolved in six ounces of warm water and soap was given in the form of enema, not so warm as when given for the purpose of clearing out the bowels, that it might be longer retained, and to avoid being deceived by the relaxing effects of heat. One hour after the enema was given, there was a decided improvement on the rigid os uteri. The vagina became soft and secreted mucus freely. The os uteri became thicker and softer in the edges, and the external parts began to relax, and dilate. At the end of one hour and twenty minutes after the antimonial enema was administered the delivery was easily accomplished.

Encouraged by the result of this case, I selected for after-experiment primiparæ, particularly the robust and plethoric, where most difficulty was to be anticipated.

In eight cases where the pains were weak, and the parts unyielding, it was of great benefit in quickening the pains, relaxing the parts and expediting delivery, saving much suffering and protracted attendance.

It can then be used with safety in the early stages of labor, although the os uteri be thin in the edges and the soft parts firm and contracted. In multiparæ, when the parts are normal, the os uteri dilating slowly, and the pains weak, it will sharpen the latter, and thus shorten the duration of labor. Flooding did not occur in any case, nor was nausea or vomiting more troublesome than in the average of ordinary deliveries. In one case (multipara) there was pain in the abdomen after the placenta was removed, but not more than is often met with in ordinary after-pains. A cup of good tea was given to the mother as an antidote, and at the same time to refresh her. Being a considerable distance from my residence I left an opiate diaphoretic to be taken if the pain in the abdomen should return. On my next visit I was told that it was not necessary. None of the cases required any particular treatment.

Dr. H. R. Storer (Boston Medical and Surgical Journal, February 14th,) corroborates these observations of Dr. Young, by reporting a very striking illustration of the relaxing influence of the tartarized antimony. Dr. Storer was called to a young woman aged 22 years, in her first pregnancy. The waters had been discharged at day break, and at 11 o'clock A. M. the os was small and extremely rigid; presentation normal, and no progress making in the labor. This continued throughout the whole day, the pains being expulsive and severe until 9 o'clock P. M. when another examination was made. The os was still rigid and had not dilated in any considerable degree since the morning—the vagina being dry and hot.

Dr. Storer at half past 9 o'clock injected one grain of tartar emetic dissolved in six ounces of tepid water into the

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rectum. In fifteen minutes, the os was found much dilated; the vagina bathed in a profuse mucous discharge. In half an hour after administering the injection, the head had reached the perineum where it was delayed some time, but the child was born by half past 11 o'clock.

3. *Statistics of Sixty-nine Cases of Labor in which Ergot of Rye was administered.* By R. U. West, M. D.

The pernicious influence of ergot when taken during labor on the fœtus has been often dwelt upon, and many believe that it acts on the child as a direct poison. Dr. West has had the opportunity of testing satisfactorily this opinion, which is certainly very commonly received; he has accurate records of more than 2,000 cases of labor, and in three hundred of these cases ergot was administered. These statistics which reached through a period of nineteen years closing with July 1853, were reported in the Association Medical Journal by Dr. West, who stated in his concluding remarks that it was *possible* to attribute one out of seventy-four still births recorded to the influence of this agent.

Since July 1853, Dr. West has again paid especial attention to this subject. He has administered ergot to sixty-nine out of nearly four hundred midwifery cases, and in the Medical Times and Gazette, he relates the results of his renewed observation.

In the whole number of cases (sixty-nine) there were nine still births. The putridity of the fœtus in two of the nine cases, proved that they had died some days before the labor came on. Another was borne by a mother who always had still born children. In another instance it was evident that the child died from pressure on the funis during the difficult delivery of a hydrocephalic head in a footling case. In the remaining four cases, all of which were difficult and tedious, there were many reasons to account for the death of the fœtus, without laying the blame on the ergot. Dr. West concludes his remarks with the declaration of his belief in the safety of this practice—an opinion based upon an experience of nearly 2,500 cases of labor, and after administering the ergot in 369 labors.

4. *A Uterus in a Man Sixty-three years old.* By Professor Langer.

The case of Professor Langer is a physiological curiosity. Professor Aranij has recently found, at the necropsy of a

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man sixty-three years old, a structure resembling a uterus, between the rectum and bladder. The man had had a "capon's voice;" beard well grown; he had lived thirty years in childless wedlock. The uterus was two-horned, ending in two large open tubes. The mesometrum (ligamentum uteri latum of the female) ended on either side in a fine doubling of peritoneum; a true ala vespertilionis, which embraced the testicles and epididymi; and, at the upper border, the end of the tube. On the left side, the uterine horn, with its tube, was dragged over by a scrotal hernia. The distance between the two testicles in the preparation is sixteen inches. A round ligament (uterine) is marked by a bundle of vessels on the right side. The uterus is connected with the upper part of the prostate. The arteries of this uterus arise, with those of the bladder, from a common arteria vesico-uterina. The organ could be easily inflated through the abdominal end of the tube. There were no strong folds in the interior, even at the isthmus. Above the isthmus, the walls of the two-horned uterus were soft, the muscular tissue loose, its mucous membrane was easily separated as a distinct layer. On a section, there were detected tubular crypts opening on the free surface.

In fine, there were distinguished three parts of this uterus: An orificial part—a glandless, thickened portion, terminating at the isthmus; and a part provided with the ordinary uterine glands, which end in two short horns, which again end in tubes. The testicles were of the normal size. The vasa deferentia ran in an oblique direction to the isthmus uteri, to penetrate the prostate. True vesiculæ seminales were absent.—*Med. Chir. Review.*

BIBLIOGRAPHICAL RECORD.

I. *Statistics of Placenta Prævia.* By J. D. Trask, M. D. White Plains, New York. (Being the Prize Essay of the American Medical Association, at its Eighth Annual Meeting.)

Any intelligent reader, who attentively examines the medical literature of our day, will at once notice how general is the tendency of the profession in all countries to gather

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statistics in the various departments of medicine; to group together as many facts and observations as a patient study and a long continued effort will enable them to collect, with the hope of deducing some valuable law from what would be otherwise an isolated observation, and thus clearing up some of the darkest realms of the science.

This laborious method of investigation, which is destined to add many important facts to the science of medicine, has not yet been received with much favor by the American profession. It is a mode of study not pleasant to us. As a people, we are characterized by a fondness for theory, a boldness and originality of conception, and which leads us entirely away from the plodding and laborious studies of the statistician. Hence, the essay of Dr. Trask, which lies before us, is something almost novel in our home literature, and will therefore attract the more attention. The author has in former years given evidence of his fondness for statistics, and for this work he deserves and has received a well merited honor.

Of the many formidable accidents to which the process of parturition may give rise, there is nothing which is so fearful as the occurrence of placenta prævia. The sudden and ungovernable hæmorrhage, the impossibility of reaching the mischief oftentimes, and the fatal termination so much to be dreaded, invests the whole subject with the highest interest. We therefore regard these tables of more than three hundred and fifty cases of placental presentation, with their treatment and result, as offering a mass of information of great value.

Dr. Trask arranges his cases into three tables:

Table I gives us the mortality occurring under the ordinary modes of treatment.

Table II embraces cases of spontaneous expulsion of the placenta *before* the birth of the child.

Table III includes cases in which the placenta was *artificially* detached prior to the birth of the child.

We will rapidly allude to the conclusions arrived at by our author.

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From the first table we ascertain that in 200 cases in which the child was delivered by turning, 59 died, being *one in three and four-tenths.*

Professor Simpson's calculation, based upon 421 cases of turning, gives even a higher ratio of mortality—*one in two and nine-tenths.*

In 50 cases of spontaneous delivery, 7 died—*one in seven and one-seventh.*

We have then the right to conclude that it is safer, if possible, to wait for natural delivery than to endeavor to hasten the process by turning the infant.

There are three methods usually employed to check the hæmorrhage until spontaneous delivery occurs: 1st, rupturing the membranes; 2d, ergot; 3d, the tampon. These statistics show but partially the value of ergot. They prove the utility of rupturing the membranes in partial presentation of the placenta, but they also exhibit the great value of the *tampon* in *complete* presentations, when from the nature of things it is almost impossible to reach the membranes, and when the os remains so unyielding and rigid as to prevent the operation of turning.

But in addition to these three modes of procedure, we have another proposition originating with Professor Simpson of Edinburgh, *to separate the placenta from its attachment to the uterus*, and either extract it, if possible, or let it remain until expelled spontaneously.

This startling suggestion, so directly at variance with our ideas of the connections between the uterus and placenta, has been strenuously resisted by some of the most eminent men of the day. Simpson contends that the hæmorrhage takes place through the medium of the placenta, and not from the uterine vessels; that as you diminish the attachments of the placenta to the womb, you diminish the hæmorrhage; and that when the connections are entirely severed, the hæmorrhage is controlled. The old school insists that the patient's safety "depends upon the integrity of the connection between the uterus and placenta," and

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that in proportion to the amount of separation is the amount of hæmorrhage. We will see what the *facts* are, according to our author.

Table II is the history of *thirty-six* cases of spontaneous expulsion of the placenta before the child. Two deaths occurred, both from diarrhœa after an interval of from eight to twelve days.

Here certainly is a vast improvement when compared with the results of the first table. In the attempt to turn, *one in three die;* when the placenta is separated spontaneously, *none die from the hæmorrhage;* and in every instance the hæmorrhage was greatly moderated or entirely ceased.

Dr. Simpson gives a larger number of cases; and united with Dr. Trask's tables, we obtain 140 recoveries and 11 deaths after spontaneous separation of the placenta, or 1 in 137.

If hæmorrhage ceases after *spontaneous* separation, may it not cease after *artificial* separation of the placenta? So enquires Professor Simpson. Let us see what table III of Trask reveals.

As we would naturally expect, the results of artificial separation do not compare with the natural process. In 60 cases, 47 recovered, 13 died, or *one in four and six-tenths*, so that we may conclude that the proposition of Simpson, whilst it will not compete with the unassisted efforts of nature in its results, yet is to be preferred to the old method of turning, in the proportion of *four and a half to three.*

This table shows conclusively, however, that the artificial separation checks the hæmorrhage. In 66 cases, more than one-half stopped flooding at once, and only *one* is recorded where there was no abatement.

Lastly—the chances for the mother's life are increased when the separation of the placenta is judiciously employed. On the other hand, the consequences to *the child* are more serious. Two-thirds of the children are born dead.

We have thus hastily given a summary of this interesting essay, which we would recommend to our readers as a valu-

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able and accurate array of the facts bearing upon the subject under consideration.

II. *The Action of Medicines in the System, &c.* By FREDERICK WILLIAM HEADLAND, M.B. &c. &c. Being the Prize Essay which received from the Medical Society of London the Fothergillian Gold Medal for 1852. Second American from the Second London Edition. Philadelphia. Lindsay & Blakiston. 1856. (Received from A. Morris.)

Many of our readers are already familiar with the very original and valuable treatise of Mr. Headland; and we can say with great propriety to those who have not yet examined it, that they will be amply repaid for their trouble.

There is no branch of medical knowledge which is less sought after by our profession than that portion which treats of the modus operandi of medicines. To know from observation or the experience of others, what is the result of the administration of a certain remedy, satisfies a vast proportion of the faculty. How this result is made to occur, why the particular drug produces these effects, are subjects of no importance to many physicians. As long as calomel salivates and tartar emetic vomits, they care not to study the philosophy of their influence, and what should be prescribed with at least a partial understanding of its method of action, is generally employed, as any quack would give a secret nostrum or a patent pill. If we do not understand the action of a medicine, it is of little importance to know its name.

To those who wish to look more deeply into this important subject, we can do no better than to suggest a careful perusal of Headland's book. The various branches of the discussion are well marked. The theories of his predecessors are respectfully alluded to, and the book is marked with a very remarkable vigor of diction and simplicity of arrangement. We cannot give the reader a better idea of this book than to present him with the author's division of the modes

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of action of therapeutic agents introduced into the stomach. He contends:

First—That the great majority of medicines must obtain entry into the blood, or internal fluids of the body, before their action can be manifested.

Second—That the great majority of medicines are capable of solution in the gastric or intestinal secretions, and pass without material change, by a process of absorption, through the coats of the stomach and intestines, to enter the capillaries of the portal system of veins.

Third—That those medicines which are completely insoluble in water, and in the gastric and intestinal juices, cannot gain entrance into the circulation.

Fourth—That some few remedial agents act locally on the mucous surface, either before absorption, or without being absorbed at all. That they are chiefly as follows:

a. Irritant emetics.
b. Stomach anæsthetics.
c. Irritant cathartics.

Fifth—That the medicine, when in the blood, must permeate the mass of the circulation, so far as may be required to reach the parts on which it tends to act.

That there are two possible exceptions to this rule:

a. The production of sensation or pain at a distant point.
b. The production of muscular contraction at a distant point.

Sixth—That while in the blood the medicine may undergo changes, which in some cases may, in others may not, affect its influence. That these changes may be:

a. Of combination.
b. Of reconstruction.
c. Of decomposition.

Seventh—That a first class of medicines, called hæmatics, act while in the blood, which they influence. That their action is permanent.

1. That of these some, called restoratives, act by supplying, or causing to be supplied, a material wanting; and may remain in the blood.
2. That others, called catalytics, act so as to counteract a morbid material or process; and must pass out of the body.

Eighth—That a second class of medicines, called neurotics, act by passing from the blood to the nerves or nerve centres, which they influence. That they are transitory in action.

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1. That of these some, called stimulants, act so as to exalt nervous force, in general or in particular.
2. That others, called narcotics, act so as first to exalt nervous force, and then to depress it, and have also a special influence on the intellectual part of the brain.
3. That others again, called sedatives, act so as to depress nervous force, in general or in particular.

Ninth—That a third class of medicines, called astringents, act by passing from the blood to muscular fibre, which they excite to contraction.

Tenth—That a fourth class of medicines, called eliminatives, act by passing out of the blood through the glands, which they excite to the performance of their functions.

III. *Materia Medica or Pharmacology and Therapeutics.* By WILLIAM TULLY, M. D. Nos. 13 and 14. Springfield, Mass. Published by Jefferson Church, M. D. 1855.

The last two numbers of this learned work are mainly devoted to the consideration of the class of narcotics. The subject is evidently a favorite one with the author, and he treats it *con amore.* We have extended catalogues of articles admitted into the different groups of narcotics recognized by the author, with ingenious observations on the peculiarities in their modes of operation. Here and there, are instructive comments on individual articles, of which many are but little known. There is some sharp criticism, and some that is amusing. The work is not exempt from repetition and verbosity, but everywhere there is evidence of erudition and research. However we may differ with the author in regard to many of his views, we none the less respect and admire his independent thought, industry and sound scholarship.

It is obvious that the author believes that the power of many of the narcotics to destroy life has been overrated. Thus, to cite a single example, he says (p. 785) of digitalis and hyoscyamus, that he "never knew or received information of a death from either of these articles," that the man-

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ner in which they destroy life is not certainly known, that "it is certain only that they do not destroy it easily;" whereas both Orfila and Wibmer record cases of fatal poisoning by hyoscyamus, and instances of the fatal effects of digitalis are contained in all works on toxicology.

Faithful to his maxim that the materia medica should consist of all indigenous and exotic articles known to be capable of being useful in the treatment of any disease, the author enumerates no less than three hundred and eighteen articles, in his catalogues of narcotics. After this, with a refinement of cruelty, he quotes John Murray's catalogue, which cuts a lame figure, as far as numbers are concerned, for it consists of only nineteen articles, nine of which, Dr. Tully maintains, possess no narcotic power whatever!

In the fourteenth number the author begins his preface to his class *Erethistica*, a new term, applied to certain remedies increasing functional activity, of which nux vomica is the type.

We are pleased to learn, from an advertisement in the thirteenth number, that the obstacles which caused a temporary suspension in the publication of this valuable addition to American medical literature, have been happily surmounted, and that the work will hereafter appear regularly in monthly parts. We beg leave to ask for it the favorable consideration of our readers. As its publication is the private enterprise of one of their number, it should naturally secure the sympathy and support of medical men.

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EDITORIAL AND MISCELLANEOUS.

Placenta Prævia.

THE great defect in all periodical literature is its miscellaneous and unclassified character. Intended to gather up month by month, the discoveries, theories and experiments of the day, it presents to the reader a quantity of valuable matter upon so many points, and of such opposite characters, that the mind, wearied by the incessant transition from one to the other, fails oftentimes to gather that amount of aliment which it would receive under a more methodical arrangement of the several subjects.

We have long intended to remedy this evil in some degree, by endeavoring to group together, as far as possible, the important essays, new publications and original articles bearing particularly upon some subject regarded by the profession as of sufficient importance to deserve their special attention. We may succeed in giving to each monthly issue an appearance of unity, by thus devoting its pages mainly to the discussion and examination of one subject; and at the same time, by a sufficient amount of miscellany and a carefully compiled and condensed chronicle, we can still preserve to the journal a variety, which will prevent it from becoming tedious and uninteresting.

A glance at the present number will explain the object we have proposed. Taking the important subject of *Placenta Prævia* as the leading idea, we offer to the student the carefully prepared synopsis of Pajot, and the very complete and well arranged arguments of Dr. Legroux. In our bibliographical department we have an opportunity of examining the original propositions of Simpson of Edinburgh and the elaborate statistics of Trask, whilst in the article from the pen of Dr. Mettauer we can see the opinions and practice of our own profession upon this important and interesting question.

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We will briefly sum up a few of the reflections which the perusal of these several views, gathered from so many sources, has suggested to us.

First—Of the tampon. English, French and Americans unite in attributing great value to the use of the tampon in placental presentations. Pajot wisely cautions against the danger of converting an external into an internal hæmorrhage; but when the membranes are entire and the uterus is full, we think it clearly proven that by a judicious application of the tampon we may often restrain the hæmorrhage until spontaneous delivery occurs. By reference to the essay of Trask we at once see that the mortality, after spontaneous delivery, is one in fourteen; whilst, after artificial delivery, the risk is as great as one in three. The medicated tampon of Dr. Mettauer is worthy of farther attention, and the india rubber globe plug, proposed by M. Legroux, is convenient and easily obtained.

The idea of Professor Legroux to produce pressure upon the placenta *from within outwards* by placing the woman in the upright position, and thus causing the weight of the ovum to bear down upon the os uteri, is original and apparently philosophical. The fœtus thus acting as an internal tampon, the external plug will be still more effectual in checking the hæmorrhage, whilst the vertical position would probably expedite the labor, a consummation devoutly to be wished.

Lastly—The startling and original proposition of Professor Simpson of Edinburgh, to separate the connections of the uterus and placenta, and thus to check the fatal hæmorrhage, is confirmed by the statistics of Trask, and assented to by Mettauer and Legroux. We may regard the fact as determined from the observations of Dr. Trask, that by separating the placenta from the uterus we may confidently expect to stop the flooding, and with almost equal certainty to kill the fœtus. If the tampon fails, and the os has not dilated sufficiently to justify us in perforating the membranes, we have in Simpson's suggestion a sure method of

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controlling the fatal hæmorrhage, and thus increasing the chance of safety to the mother at the expense of the child.

Army Meteorological Register.

THE wise requirement of a careful and extended system of meteorological observations from the medical staff of the United States army has resulted in the publication of the "Army Meteorological Register" for the past twelve years, which has been prepared by Dr. Coolidge under the supervision of Surg. Gen. LAWSON, to whose politeness we are especially indebted for a copy of this truly magnificent work.

A series of accurate observations of the temperature, winds and weather, taken at, probably, one hundred and fifty points scattered throughout our wide spread confederacy, must result in an amount of climatological information of the greatest practical value. Reaching from the advanced posts of the United States army on our extreme northern border, to the semitropical climates of Florida and Texas; thence throughout the remarkable region of country separating the Atlantic from the Pacific region, including New Mexico and the great Salt Lake region, and extending as far as Oregon and California, these details present a most extended field for the study of the laws of climate, and will lead to the development of important general principles.

Accompanying this report, which is published in a style worthy of the government under whose direction the observations have been ordered, are a series of isothermal charts giving the mean distribution of temperature throughout our country, and also a system of rain charts, by which we can compare the relative distribution of rain in all portions of the United States. These maps have been arranged by Professor Blodget, late of the Smithsonian Institution, and add much to the usefulness of the work.

We hope to give our readers in a future number, some of the interesting deductions which a more careful study of this elaborate work will enable us to obtain.

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Conservatism in Medicine.

To counterbalance the many disagreeable circumstances connected with the performance of his duties, the medical journalist enjoys some peculiar gratifications. At almost every issue of his periodical, it devolves upon him, for example, to announce the pleasing intelligence that some disease, hitherto regarded as almost uniformly fatal, may be readily cured, or to report some new therapeutical agent whereby curable diseases may be treated with all the advantages and none of the disadvantages of antecedent remedies. What can be more delightful to the philanthropic editor than to be instrumental in informing the attendants on afflicted humanity of the miraculous results of all these new discoveries? With a preparatory flourish of trumpets, in the form of a preface, he heads the triumphal procession as it were, that declares the glad tidings of the prevention and cure of phthisis. With many notes of admiration, he records methods for the extermination of cancer. He presents epilepsy shorn of its terrors, and publishes a multitude of specifics for cholera. He is able to assure his readers that, when their patients have scarlatina, it is only necessary to grease them; when they have whooping cough, to give them nitric acid; that diabetes may be cured by blisters and cups at the nucha, and tetanus, by cold water and ice along the spine! The editor's only trouble in this matter is to comprehend how, when so many specifics exist, any more should be wanted. He is astonished to observe, as he turns over his exchanges, that each writer disregards the discoveries of his predecessors, and ushers in his own marvelous remedy with expressions of regret at the inefficacy of the rest. He is forced to explain the striking contradiction by the disrespectful supposition that the profession experiences, in regard to the innumerable specifics laid before it, the feeling that the ass did betwixt the bundles of hay. Doubting which to prefer, he died of absolute starvation. Otherwise it is impossible to reconcile the unabated fatality of some diseases, and the obstinacy of others, with the accounts of their triumphant success reported by medical authors.

Seriously speaking, we ourselves, like our colleagues in journalism, endeavor to put our readers in possession of all the so-called novelties. Of many of them, we might say with Martial: *Nos hæc novimus esse nihil;* but we do not, on that account, feel at liberty to withhold matters of current professional interest. We add a word of caution sometimes, as, for example, in mentioning, last year, that hundreds of cases of yellow fever had yielded to the tincture of sesquichloride of iron, we stated in a postscript, that the discoverer of this specific had died of the fever. Or we offer a brief criticism, pointing out perhaps that the last remedy is vaunted at the expense of other and familiar ones of equal or superior

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power. It is not desirable, however, that we should be continually obtruding our own observations on the reader; and consequently we shall beg leave to take advantage of this corner of the journal, appropriated to small matters, to beg the brethren not to hold us responsible for the assertions and opinions of which we are interpreters simply. We are not, if a little egotism may be permitted us, the advocates of every new method or theory, but own rather to a certain fogyism and respect to old principles and traditions. We fully assent to the doctrine expressed in the following admirable language of Sir Benjamin Brodie:

"I am not one of those who would be trying indiscriminately all the new remedies, which, in these days, are being constantly brought before the public; nor can I think well of this modern fashion of resorting on all occasions to novel methods of treatment. I see many practitioners who would always rather give a new medicine than an old one; but I advise you, if you wish to succeed in your profession and to be useful to the public, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and, as a general rule, have recourse to them first. If the old remedies fail and you are at a stand-still, then, and not till then, have recourse to the new ones. If you always begin with the new remedies, you throw away all the valuable results, not only of your own experience, but of the experience of those who have gone before you. You have to begin as it were *de novo*, and the first consequence of this will be that you will not cure your patients; and the second, that you will have none to cure. When old remedies fail, I say it is not only not unreasonable, but proper, that you should ascertain what can be done by new ones; but it is very unwise to employ the latter where there are sufficient grounds to believe that those already in use will answer the intended purpose. I should be very sorry to see the march of science impeded by an unjust apprehension of experiments and innovations; but, surely, there is a broad enough line between a discreet and prudent use of new remedies, and that indiscreet and hasty use of them which we find to prevail in the practice of the medical profession at present."

It is better that a few new facts or useful innovations should make their way slowly, than that science should be incessantly disturbed in its foundations, and have the truth of all its acquisitions every now and then called in question. It is probable that the most bigoted opposition has never repressed a real improvement or great truth for any considerable length of time. Better, then, that the Galileos and Jenners should undergo bitter trials, than that the edifice of human knowledge should be continually shaken.

Medicine can tolerate less mobility than other sciences, for its issues are applications to practice, of which the human body is the theatre; this is a sufficient reason for exacting a religious severity in the trial of novel facts and theories.

Let us remember that all great physicians have uniformly based their practice on a few simple principles. Although besieged by the impa-

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tience and curiosity of their juniors, and worried by the presumption of inexperienced innovators, they have been steadfast in their principles. Let us not attribute this course altogether to obstinacy and pride. Let us do justice to this spirit of resistance; it is to it that we owe the maintenance of the traditions of our art. If the restraining wisdom of our foremost men did not balance the new ideas that constantly arise, every century would have to reconstruct the temple of knowledge; science, like the ephemera which die the day they are called into existence, would scarcely survive a single generation, and its doctrines would be subjected to change as constant as the habits and models of fashionable life.

VARIETIES.

Medical News and Items.

ANÆSTHESIA AN OLD AFFAIR.—In Middleton's tragedy of "Women, beware, Women," published in 1667, there is the following passage:

> "I'll imitate the pities of old surgeons
> To this lost limb, who, ere they show their art,
> Cast one asleep; then—cut the diseased part."

We copy this citation from a newspaper, and being unable to look up the reference, have some doubts of its accuracy. Thomas Middleton, the dramatist, was a contemporary of Fletcher and Massinger, and must have died long before 1667.

REGISTRATION.—We rejoice to learn that a proposition is before the Maryland legislature, for the enactment of a law for the registration of marriages, births and deaths.

PAR NOBILE FRATRUM.—From our valued exchange, the New Jersey Medical Reporter, we learn that two graduates of the University of Pennsylvania, the *Hon.* W. A. Newell, M. D. and A. D. Newell, M. D. of New Brunswick, N J. who claim to be *regular* physicians, have appended their certificates to the advertisement of an anti-periodic nostrum, the active principle of which is understood to be quinoidine. The *Honorable* and his colleagues have used the pills "in hundreds of cases," and "take pleasure in stating thus publicly their experience of their efficacy." In another paragraph, the *Hon.* Theodore Frelinghuysen certifies to the eminent positions occupied by his friends, the Newells, and to the reliability of their statements. We wish the candidate for the vice-presidency of the United States joy in his new vocation. A little further exertion, and he will obtain, perhaps, an invitation to dine with Dr. Townsend, or, the vice-presidency of the society for the propagation of nostrums in foreign lands. One of the culpable physicians boasts that he is still a member, in good standing, of the Monmouth Medical Society. Now, it is worse than useless to

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inveigh against quacks and quackery outside of the profession, so long as these things are tolerated in it.

MALPRACTICE.—Another of those lawsuits which of late years have recurred in New England with disgraceful frequency, is reported in the Massachusetts journals. A man brought an action for damages of $5,000 against Nathan Allen, an estimable surgeon of Lowell, in consequence of alleged malpractice in the treatment of an injured arm. The case was tried in January, at Cambridge, before Bigelow, J. and a verdict was rendered exonerating the surgeon. We would suggest that if ever the plaintiffs in such suits as these, or the rascally attorneys who instigate them, should have strangulated hernia, or stone, or a foreign body in the air passages, they should be left to their own devices.

MEMORIAL OF THE MARTYRS.—The relief committee of Philadelphia, representing the contributors who so nobly assisted the suffering poor of Norfolk and Portsmouth in the late memorable epidemic, has determined to erect, in Laurel Hill, a monument to the memories of the physicians, nurses and druggists of Philadelphia, who perished in their humane mission to the plague-stricken cities. The design, as shown in a pamphlet published by the committee, is by Mr. Baird, and represents a Roman Doric column, twenty-five feet high, surmounted by a draped funereal urn. On the foundation is a pedestal of Carrara, enriched with *bassi relievi* of the Good Samaritan, Hippocrates refusing the Bribe, Charity, and Laying out the Dead. There are several inscriptions; among others, this, from the twenty-fifth chapter of St. Mathew:

"I was sick and ye visited me."
"Verily I say unto you, in as much as ye have done it unto one of the least of these my brethren, ye have done it unto me."

And, on another face of the pedestal, the names of the martyrs:

Robert H. Graham, nurse, 27; Thomas W. Hardy, druggist, 19; John O'Brien, nurse, 42; E. Perry Miller, druggist, 21; A. Jackson Thomson, nurse, 26; Dr. Courlten Cole, 31; Mrs. Olive Whittier, nurse, 55; Dr. Thomas Craycroft, 30; Singleton Mercer, 35; Edward R. Barrett, student, 28; Frederick Muhsfeldt, nurse, 42; Henry Spriggman, nurse, 49; Dr. Herman Kierson, 28; Miss Lucy Johnson, nurse, 25; James Henessy, nurse, 54.

Foreign Items.

We have received our files of foreign journals on the eve of going to press. We have not time to make many extracts, but glean a few items of news.

The discussion on *Setons* at the French Academy of Medicine has become an extended debate on the theory of revulsion and derivation in general. We have interesting discourses from MM. Bosquet and Parchappe, distinguished representatives of the vitalist school, who have rarely mingled of late in the academic discussions.

The death of M. Foderé having left a vacancy among the *corresponding members* of the institute, it was resolved by the learned company to assign this honor to some foreign medical man. The section of medicine and surgery, being

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instructed to report candidates, handed in the names of Marshall Hall, Rokitanski, Christison, Riberi, and Chelius. Marshall Hall received, at the first ballot, all the votes but two, and was declared elected. This post is second only to that of *foreign associate.*

The institute has nominated as candidates for the late Professor Magendie's chair at the College of France: 1. Claude Bernard. 2. Longet. 3. Brown Séquard. M. Bernard will be appointed, but we are glad to see our friend Dr. Séquard in the way of promotion.

Candidates for Magendie's place in the Institute are pouring in. The names of Jobert, Maisonneuve, Cruveilhier, Brown-Séquard, Guérin and Longet are prominently mentioned.

Professor Muller has been shipwrecked, and narrowly escaped drowning. A steamer, in which he was returning from Norway, was run down and sunk by a larger vessel. The illustrious physiologist, being an excellent swimmer, kept himself above water until assistance reached him.

The cholera ceased at Madrid early in November. The Semanario states that the number of persons attacked during the epidemic of 1855, in the capital, was 5,501. Of these 3,697 died—an awful mortality. The Queen of Spain has issued a letter of thanks to the physicians of Madrid for "their devotion and abnegation during the pestilence, their faithful christian charity, and love for their honorable profession." Her Catholic Majesty has directed that the sum of 1,000 reals, (about 50 dollars) shall be given to each of the widows of physicians who died in the discharge of their duties during the epidemic, and regrets that the state of the treasury will not allow her to do more.

Professor Bérard has had a stroke of apoplexy, but is recovering.

On the 11th December, the Academy of Medécine held its anniversary meeting. M. Dubois of Amiens, panegyrist in ordinary to the academy, pronounced an eulogy on the late Professor Recamier. Dr. Depaul read a report on the prizes adjudicated this year. The "Academic prize" of 1,000 francs, for the best essay on "the influence of change of locality, such as removal to a warm climate, or a sea-voyage, on the progress of tubercular disease, determined by precise facts," was assigned to Dr. Rochard, a naval surgeon. Baron Portal's prize, of 1,000 francs, was divided between three essayists. The subject was "goitre." The "Itard prize" of 3,700 francs for the best recent work on practical medicine on therapeutics, was given to Vidal (de Cassis,) for his treatise on venereal diseases, which was reviewed last year in this journal. The academy had offered a prize of 1,500 francs for the best memoir on the action of alkaline mineral waters. This was carried off by Petrequin, the eminent surgeon of Lyons. The "Civreux prize," 1,000 francs, question "Catalepsy," was divided. The "Capuron prize," 1,000 francs, question "Sudden deaths in the puerperal state," was reserved, the essays being unworthy of recompense. M. Bussy was elected president for the ensuing year, this honor being due, according to the academic principles of rotation, to the section of pharmacy and medical chemistry. Dr. Depaul was re-elected annual secretary.

The faculty made their accustomed "*rentreé*" in November. Malgaigne pronounced a brilliant eulogy on Roux.

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purpose; a good constitution, so as to be able to sustain any fatigue and depri-
CONFIDENCE OF HIS PATIENT.
hysicians, a forceps, some ergot freshly powdered, and half an ounce of laudanum.
al emotion and cessation of the uterine contractions.
Whether there has been lately a passage of the urine or fœces.
etter to wait, in order that he may judge for himself.

e often troubled with false pains, depending upon a sympathetic disturbance of the
radually moderate to the region of the perineum or anus. The first are generally
e neck, the tension and protrusion of the membranes at a contraction, the relaxation
THAT SHE HAS ARRIVED AT THE FULL TERM. 4th. WHETHER THE MEM-
UTH AND NECK OF THE UTERUS, THE VAGINA, THE PERINEUM. 7th. WHE-
HE CHILD IS ALIVE OR NOT, (*auscultation, flaccidity and softness of the skin and*
iotic fluid.) (A mucous discharge from the vagina (the shows) generally makes

rm, there is danger of hæmorrhage or convulsions; if too cold, of subsequent inflam-

he abdomen of the mother, some olive oil or sweet grease, plenty of warm water
ll not probably be needed, but it is better to have them at hand.)
t the wall. One mattress should be laid upon it in its whole length, and upon it
ordinary bedstead, and the one upon which the woman is to remain, the sheets
covered as before with a sheet.
tress, the lower extremities being extended upon the oil cloth laid upon the
f the bed.
under the arms and shoulders, to prevent soiling, and the lower limbs should be

h it by her own efforts, practice catheterization.

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latation; but after a period of suspense, it will commence again. It is not so often

given above, constitutional weakness, a deranged condition of the digestive organs, f the patient, open the bowels, by slightly stimulating enemata, and for the plethora after rest, the pains do not return, ADMINISTER ERGOT.
nly in the second period. If the inclination is forwards, the woman should lie upon difficulty.

after a time. If not, the membranes must be ruptured.

r, during a pain, and when the membrane is tense; but this course should never be e vagina.

h prolonged, it will be accomplished naturally.

If there is danger of laceration of the uterus, bleed to 15 or 20 ounces, and give *he os, recommended by some authorities, should be resorted to only in extreme cases, and*

contractions get weaker, or the intermission becomes longer, or they may cease Administer ergot, or if prompt delivery is demanded, use the forceps.
bring on the contractions again. (The patient is often much encouraged by being frequency and force.)

BY THE SOFT TISSUES.

that nature will effect a dilatation. If a minute opening is found, but there is no and there is danger of a rupture of the uterus, open by an incision or incisions by the healing of some old ulceration, whether before or after conception, or by a

e is no tendency to dilatation, use the knife as before, and if necessary, version or

ion by the hand and assist

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accidental complication.)

COMPILED BY NATHAN P. RICE, M. D.

ngth by turns around the body, neck or legs of the fœtus. The shortness can eparation of the placenta;) induce hæmorrhage by tearing away the placenta, y must be effected.

in becoming so compressed between the fœtus and the walls of the uterus or

leviation of the pelvis; 4th, an obliquity of the uterus; 5th, a malposition or the membranes and escape of the fluid—ONLY DANGEROUS FOR THE FŒTUS, that the delivery will be soon accomplished, or if the head is small and the ER AT ONCE WITH THE FORCEPS. Some authors have recommended the the child, but both acts are difficult and dangerous.

on, contraction of the circular fibres, making a cylinder,) OR MORBID ADHESION ed, or if there is hæmorrhage, we should give ergot, or at once extract it with To extract the placenta, the right hand should be introduced conically into the cut the leaves of a book with a paper knife. Cazeau.) The left hand should ndeavor to avoid dangerous hæmorrhage. (INJECTION OF THE FUNIS with cold

characterized by the ordinary peculiarities of hysteria, (*previous hysterical or* abor may be stopped at each paroxysm, but will commence after its cessation. upon the head and chest, are our best remedies.

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VIRGINIA MEDICAL JOURNAL.

THE STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL

COMBINED.

APRIL 1856.

ART. I.—*A Report on the Eruptive Disease lately prevailing in New Kent County.* Submitted to the Hon. HENRY A. WISE, Governor of Virginia. By WILLIAM A. PATTESON, M. D. Richmond.*

RICHMOND, March 1, 1856.

DEAR SIR,

Having received from the secretary of the commonwealth an executive order, dated 25th February 1856, referring certain letters and papers received from the county of New Kent, and the letter of Dr. Arthur E. Peticolas to me, and directing me to repair to the county of New Kent and ascertain whether the cases reported to be vario loid or small-pox in that locality, are such, and also to enquire whether they have been occasioned by any neglect or mismanagement of the vaccine agent, and report thereupon

* During the month of February last, various communications were received by Dr. A. E. Peticolas, the vaccine agent in Richmond, charging him with having transmitted small-pox instead of vaccine virus to several persons who had made application to him, the result of which was that many persons had been inoculated with small-pox matter. During the same month a petition was for-

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to the governor—I have now the honor to say that I have duly executed the order, as will more fully and at large appear by the following detailed account.

On Tuesday, the 26th February 1856, I repaired to the said county, and was kindly and professionally met by Drs. Robert S. Apperson, Vernon P. Jones, Wm. Pryor Richardson, A. J. E. Jennings and some other citizens of the vicinity of Barhamsville.

I learned from them that the small-pox appeared in the county of New Kent some time about the second week in January last, in the person of a white man named Thomas Breeden, about 21 years of age, who was a trader to Richmond. He sickened with chill and fever, and was visited by Dr. Vernon Jones and Dr. James Richardson. After a few days, an eruption appeared on his body, which was at first thought to be measles. In a very short time, it was ascertained to be small-pox of a dangerous kind, and he died on the 28th day of January. There then sickened at and about the same time, in this house, George W. Mitchell, the head of the family, his wife and infant daughter, Mrs. Breeden, the wife of the deceased, her son Lloyd, Joseph Robbins, a boy of 12 years of age, living in the family, Pryor, a free negro lad, aged about 18 years, and Peter, an old man, aged about 70 years, the property of John G. Mitchell.

Of these, Pryor, Lloyd, Robbins and old Peter had small-pox. The rest had varioloid of a mild form, having been previously protected by vaccine, except the child, which also had varioloid. These were all inmates of this house.

warded to the governor from New Kent county, desiring him to remove the vaccine agent from office on account of neglect of duty, in as much as forty or fifty persons had been afflicted with serious disease resulting from the use of matter received from the agent, purporting to be vaccine virus.

Dr. Peticolas at once submitted these papers to the governor, who, with commendable alacrity, directed Dr. Patteson to visit the county of New Kent, and report to him on the condition of the sufferers in that locality, and also to ascertain whether the agent was chargeable with any culpable mismanagement or neglect of duty.

In pursuance of the order of the executive, Dr. Patteson submitted the interesting report which we now lay before our readers.—*Editor.*

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After these occurrences, a hospital was established by the magistracy of the county on the 4th day of February, and Drs. Vernon Jones and Wm. Pryor Richardson appointed physicians thereto. Mitchell's house was constituted said hospital. Dr. Wm. P. Richardson had not visited it before this date. It is said to be about five miles from Barhamsville, not on the public road, but in an out of the way place, and the communication with it and the surrounding country was interdicted. Interdiction with it had been established, by common consent, some time before it was constituted the hospital, almost directly after the eruption appeared on Breeden.

Thus much as to circumstances and facts attending the origin of small-pox in New Kent, on the information of others, communicated to me.

On Wednesday, the 27th, and Thursday, 28th February, I visited, in company with most of these physicians, the families of Mr. Leroy Williams, Thomas Boswell, Mrs. Sally Hockaday, Thomas A. Digges, Robert S. Apperson, Thomas Turner, Jno. A. Richardson, William O. Hockaday, Thomas M. Timberlake, John S. Lacy, and John M. Timberlake, and I found the following to be the attending circumstances:

On Wednesday, the 6th February 1856, some of the virus received from the agent by Mr. John A. Richardson, was inserted into the arm of Mr. Leroy Williams by Dr. A. J. E. Jennings. On the 15th, nine days after the insertion, he was attacked with chill, sick stomach, severe pain of his head and back for several days, and fever, which was followed on the Monday following, being 12 days from the insertion of the virus, by abundant eruption over the face and most of his body and extremities. His face was so swollen, he reports, as to diminish vision. His fever abated on the coming out of the eruption, and the pustules soon began to dry up. This day, 21 days from the insertion of the virus, the original scab is holding firmly to its seat, dry and hard, darker than pure vaccine in the white race, and many hard, horny crusts in the neighborhood, and on other

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parts of his body. The stains of hundreds which have fallen are to be seen. He had been formerly vaccined, probably 30 years ago; and two pits on the same arm, with the characteristic marks of vaccine, are distinctly visible. It seems to me to have been a case of severe varioloid.

Ellen, his daughter, about 12 years of age, had virus inserted in her arm by Dr. Wm. P. Richardson. This virus had been received from the agent by Dr. A. J. E. Jennings, who gave it to Dr. Richardson. She sickened on the 9th day after the insertion of the virus, with chill, fever, sick stomach, vomiting, headache and slight backache. The eruption came out three days after the access of fever, fully; her fever abated; had hard, horny pocks, which have desquamated nearly, leaving stains, but no pits. The original scab has fallen, leaving a pit. She had not been previously vaccined.

Elizabeth, a black girl about 14 years old, had some of the same virus inserted in her arm at the same time by Dr. Richardson. Her case went on simultaneously with Ellen's, with like symptoms, and presents now similar characteristics. She will be pitted a little. They are cases of *varioloid* produced by this virus, on subjects *not* protected by previous vaccination.

In the same family, no others had any of the original virus inserted; but Mrs. Williams on the Tuesday following the access of fever in Ellen, before the eruption appeared on her face and body, took matter from her arm and inserted it into the arms of four negroes, three of them young, and a woman. Two of them have had a like disease, and all of them have been sick. Dr. Richardson, a few days after the insertion of this virus by Mrs. Williams, and before any symptoms of sickness had come up, vaccined all four with other vaccine virus, not derived immediately from the agent. It was matter taken from the arm of one of Dr. Jones' servants, which servant had been vaccined with matter sent to Dr. Jones by the agent, which proved to be genuine vaccine.

Two of these negroes so treated had about as much disease

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as Ellen and Elizabeth, namely, boys Jack and George. The other two, Lizzy and Drusilla, were very little sick; slight fever, and no eruption. Vaccine seems to have taken perfectly in these two. It seems to have arrested the inoculation made by Mrs. Williams entirely, and the vaccine crusts are perfect on Lizzy's and Drusilla's arms. The modifying influence of Dr. Richardson's vaccination was much less with Jack and George. They passed through about the same curriculum of symptoms as Ellen and Elizabeth, and have had decided varioloid, with a full crop of eruption over the whole body, and are now covered with horny scabs. The crusts of vaccine made by Dr. Richardson are still holding on the arms of both these boys; and if removed and used, would doubtless produce just such a disease as is now prevailing in New Kent, though no man could distinguish them from genuine vaccine crusts. So also of the crusts on the arms of Lizzy and Drusilla.

In the family of Mr. John A. Richardson, his son George, a lad about seven years of age, had inserted into his arm by Dr. A. J. E. Jennings, on the 6th day of February, some of the same matter used by Dr. Jennings on Mr. Williams. He was seized with chill on the ninth day after its insertion, being the 15th day of February, followed by high fever, pain in his head and back, vomiting and delirium. Three days thereafter, being the 18th of February, full eruption came out over the face and body, with fierce consecutive fever. He presents now all the appropriate phenomena of small-pox. The pustules now this day, 27th February, have burst, and are crusting on his face, and he begins to look about and take food. They are large, hemispherical, and full of matter on the body and extremities; his feet and hands swollen and very painful. *It is true small-pox*, in all its essential characteristics, and is an evidence of the capacity, of what I suspect to have been varioloid virus, when introduced into the system, to produce true small-pox.

Susan, child of Mr. John A. Richardson, had some of the same matter used with her by Dr. Jennings. It failed, being

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slightly executed. She has since been vaccined with other matter, on the day George had chill and fever. It took favorably, and she is now well, running about the room in which her brother is sick, it being the chamber, and a mature, perfect vaccine crust on her arm.

His infant was successfully vaccined the same day with some of the same vaccine virus used on Susan, and now has a perfect vaccine crust on its arm.

In the family of Mr. Thomas Boswell, Dr. Jennings used some of the same matter received by him, on Mr. Boswell's son and daughter, aged about 17 and 13 years, and a young man in the family, named Lewellin Crowdis. They all three sickened with chill, fever, headache and backache from about the 9th to the 11th days after the insertion of the virus, and passed through disease of various grades.

The daughter sickened first, and had a mild attack of distinctly marked varioloid. The son presents the appearance of a combination of discrete small-pox and varioloid. Some few of the pustules are hemispherical, and filled with yellow pus, but the great proportion are hard, horny varioloid crusts. He will be pitted a little, and is nearly well.

The young man Crowdis has true small-pox, with all its distinctive marks. It is crusting on his face, but is full and abundant on his extremities. He suffered from high delirium, and was very sick. The virus used on these subjects was part of the same parcel received by Dr. Jennings from the agent, some of which he had given to Dr. Wm. P. Richardson, and was used by Dr. Richardson at Mr. Williams' on Ellen and Elizabeth.

In the family of Mr. William O. Hockaday, the virus, which was sent him by the agent, in the envelop directed to Dr. Rob't S. Apperson, was inserted by himself into the arms of 17 members of his family. It produced varioloid of slight character in most of them, but a smart attack in Charles, Dick and George. Lewis, Washington, Caleb and Spencer were scarcely if at all sick, having simply sore arms, having been heretofore vaccined. Mrs. Hockaday and three young

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children have had slight varioloid, but her infant has had true small-pox. The crusts have mostly fallen, and it is nearly well. It is the only case in this family of true small-pox from the matter received.

Miss Mary Emily Parish, a member of this family, was absent from home when the matter was used with the others, and on her return was inoculated from Charles' arm, who had varioloid incubating, as his subsequent eruption has proven. This was done on the 13th February. She now has small-pox of the distinct form. She had ague last Friday, and now, five days since, is covered with distinct small-pox, which is filling up. She had never been vaccined previously.

The virus received by Dr. Jennings was used in the family of Mrs. Sally Hockaday. A colored man, named Washington Scott, had it inserted on the 8th February 1856. He has been quite sick, having had, as reported by Dr. Jennings, the usual heretofore described symptoms. He now has, 19 days since the insertion of the virus, a full crop of hard, dry, horny crusts over his whole body, which are rapidly falling. His partner in the blacksmiths' shop, whom I have not seen, is reported to have had about a similar disease. Various negro children in the same family, on whom virus taken from the arm of Mr. Hockaday's man Charles was used, have had, and have varioloid.

The virus sent to Dr. Vernon P. Jones by the agent, was given by him to Mr. Thomas M. Timberlake, who used it on 14 members of his family. They have most of them had varioloid mildly. A small negro child only has had varioloid very severely, closely approximating small-pox; and a woman, on whom virus taken from this child was used, now has varioloid. The matter was taken from the child's arm before its eruption appeared.

At Barhamsville I saw Mr. Wm. B. Vaiden, on whom Dr. Richardson used some of the same virus used on Ellen Williams and Elizabeth, and on the same day. He took no disease therefrom. He had previously been vaccined, and presents a perfect vaccine eschar.

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A son of Mr. Timberlake, likewise protected by previous vaccination, has resisted the matter used on him, which was taken from Mr. Timberlake's little negro, which was so severely sick.

In the family of Mr. Thos. Diggs I saw negro girl Ellen, on whom some of the same matter received by him was used by Dr. A. J. E. Jennings on the 6th day of February 1856. Drs. Jennings and Jones report that it was ushered in with all the symptoms heretofore described. Now, 22 days since the insertion of the virus, she is crusted and desquamating on the face, and beginning to do so on the body. It is a marked case of true small-pox.

In the family of Mr. John S. Lacy I saw two negroes, Fanny and Bob, about 12 and 10 years of age. Virus was used on them by their master, which virus Dr. Jennings gave him, and was part of that received by Dr. Jennings from the agent. They both sickened, Mr. Lacy says, about eight or ten days afterwards; had chill, high fever and head and backache, when the eruption appeared, and the fever began to abate. They both now have on their bodies a full crop of hard, dry, varioloid crusts. Another negro, on whom the same virus was used at the same time, escaped infection.

Dr. Robert S. Apperson, on the 1st day of February, 1856, received from the agent an envelop containing virus. In it there were four distinct small packages, and the letter advised that they were for Dr. Robert S. Apperson, John A. Richardson, Thomas J. Turner and Wm. C. Hockaday. He used it the next day on some eight or ten members of his family, of which only three were impressed—his little son Robert, about three years of age, and two small negro girls, Elizabeth and Jane. In about eight or ten days the girls had sore arms and fever, followed by smart eruption on the hands and arms of one of them. The other only had the pustule on the arm at the place of insertion. On the Wednesday week next ensuing, which was 12 days after the insertion of the virus in Robert's arm, it having inflamed but slightly, he reinserted virus in his other arm, taken from the arm of the girl Elizabeth, which was advanced, and then on

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the next day he found his son with hot fever, which continued severe until the Saturday following, when the eruption came out over his face and other parts of his body, being 13 days from the first insertion of the virus. His fever then declined, the pustules became larger and hard, have now nearly all dried, and most have fallen. The child may be considered well. He has had varioloid.

On Tuesday, 5th of February, he used some of the same virus in the family of Mr. John M. Timberlake, on five subjects. Of these only three were impressed, and they sickened about the 9th and 10th days afterwards, with chill, headache, backache, sick stomach, talking idly during the fever, some delirium, followed by full crops of eruption in three or four days. The three which sickened were Mr. Timberlake's son Edgar, colored girl Nicey, and Christiana, a servant of Mr. John R. Williams, then at Mr. Timberlake's. Nicey is Dr. Apperson's own slave, hired to Mr. Timberlake, and was very sick. She is now, the 28th February, twenty-three days since the insertion of the virus, covered with the crusts of true small-pox. She is sitting up and desquamating. She suffered much from vomiting and delirium during the fever.

Mr. Williams' girl Christiana, whom I have not seen, inoculated at the same time, is reported to me to have had a full crop of hard pustules, which crusted rapidly, with abatement of fever after the eruption. It was doubtless varioloid.

Mr. Thomas Turner used the virus enclosed in the envelop to Dr. Robert S. Apperson for him, on his family, on Sunday morning, 3rd February. He used it on four of the children of his wife, and three blacks. Three or four days afterwards, he used the same matter on six other members of his family, to wit, himself, his wife, and four blacks. His infant was afterwards vaccined with matter from another source, and has true vaccine now. It has been saved from the contagion to which it had been exposed, by vaccine. Three of those on whom the virus was used have escaped altogether, to wit, himself, a negro man and negro woman, all three having

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heretofore been vaccined. His wife had but few pustules, and the others eruptions of various degrees of violence and amount, preceded by chill, fever, sick stomach, head and backache. They now have varioloid crusts on many parts of their bodies. Only one, Sally, a negro woman who had the virus inserted on Sunday, the 3rd of February, one of the first seven on whom it was used, has *small-pox*. The eruption was very abundant over the whole body, confluent on the face, and is now, the 28th of February, and twenty-five days from the inoculation, drying safely, and desquamating. She is sitting up. It was considered, as it is, small-pox by her physicians, Drs. Jones and Richardson, who, together with Drs. Apperson and Jennings, visited these patients.

Having now, sir, duly discharged the trust committed to my charge by your executive precept of the 25th February 1856, having examined the letters and papers from the county of New Kent, and the letter of Dr. Arthur E. Peticolas to you, dated 23rd February 1856, and having, as you will see, visited and carefully examined the families and cases aforesaid, and noted the facts as they were presented to me, I have come to the following conclusions:

That small-pox appeared naturally in New Kent about the second week in January.

From close and particular enquiry, I believe that those families reported on, have had no intercourse with the first subjects of small-pox in New Kent. They not only forbade all communication with them, but the places of residence, and infantile ages of many of those now sick, forbid the idea that their sickness has been produced by any connection with those first subjects.

In reasoning from consequences to causes, it is clear that these cases are all to be traced back to the use of matter received by Dr. A. J. E. Jennings and Dr. Vernon Jones, and the four small packages covered under the envelop to Dr. Robert S. Apperson for himself, Richardson, Turner and Hockaday from the vaccine agent.

Its use has produced small-pox, varioloid, and apparent

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vaccine, the sickness of all being the direct sequitur of the use of the virus, about the usual time and in the usual manner.

I suspect and have but little doubt, that it was a vaccine crust cropped from the arm of some patient who had been exposed to the contagion of small-pox, in whose blood small-pox was incubating when vaccined, and sufficiently matured to modify it. Such crusts are not distinguishable from genuine crusts by any known law, except trial.

The agent must have become possessed of some crusts of modified virus, from his sources of supply, other than his own rearing.

His frank letter to you clearly shows the impossibility of answering the public demand for virus in time of panic, from croppings under his personal observation alone, and therefore the possibility of mistake.

There are no other modes of supply known, than those to which he has had recourse, in times of panic.

I do not see that he has been guilty of any neglect or mismanagement in the discharge of his public duty.

Very respectfully,

W. A. PATTESON, M. D.

Hon. H. A. Wise,
Governor of Virginia.

Art. II.—*Report of the Special Committee of the House of Delegates on the Vaccine Agency.* By J. Herbert Claiborne, M. D. Delegate from Petersburg, and Chairman of the Committee.

The committee to whom was referred the communication of the governor of this commonwealth, relative to the conduct of the vaccine agent, have had the same under consideration, and have examined the documents appertaining to the subject—the letters of certain persons in the county of New Kent, complaining that the agent had distributed among them the matter of small-pox instead of vaccine matter—

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together with the communication of Dr. Patteson, who was despatched by the governor to New Kent to enquire into the case, and other papers in the possession of the house, germain to the whole, and beg leave to submit the following report:

The committee have found it to be well established, that Dr. A. E. Peticolas has been the unfortunate agent of dispensing, to sundry persons in the county of New Kent, matter, which, by inoculation, has produced variola and varioloid instead of the vaccine disease. Of this *fact* the agent himself is convinced, and avers it in his candid and impartial communication to the governor. It remains, then, for the committee to enquire into the motives of the agent—whether the distribution of this noxious matter was intentional on his part—and into his official conduct—whether it has been marked by sufficient carefulness and by intelligent precaution.

In the total absence of any *reason* why the agent, a state officer of high character and unimpeached integrity, should intentionally lend himself to the fiendish act of spreading a fatal and loathsome disease among the people of this commonwealth, the committee cannot conceive it possible that his *motives* should be impugned by any one. They deem it unnecessary to say more on this point.

In reference to the official conduct of the agent, the committee believe that he has exercised as much caution and sagacity in the procurement of the matter, which he distributed to the citizens of New Kent, as he has done in all other cases when called on for matter. And that he has, in all other cases, acted with care and prudence, will appear from the fact that in a term of five years' service, in which he has supplied many thousand people with virus, he has never before been accused of sending improper matter. And moreover, it will appear, on an investigation of the mode in which Dr. Peticolas procures the vaccine scab, that no man can do more to obtain and transmit that which is genuine.

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In his communication he states that he always sends, when possible, a scab that has matured on a healthy subject under his own supervision, and that, when, from unusual and numberless demands on him, as in times of excitement about small-pox appearing in a neighborhood, he finds it impossible to furnish sufficient matter of his own raising, he applies only to such persons for his supply as are well known to him individually, and that he seeks it, without regard to price, when it can be proven to be genuine. These statements are confirmed by the testimony of competent witnesses in any court.

The committee refer you to the certificates of A. A. Coleman and D. S. Garnett, resident students of the Richmond alms-house; to the certificate of Theo. P. Mayo, M. D.; to the certificate of Messrs. Laidley & Robinson; and the certificate of Dr. F. H. Deane, former vaccine agent for the state.

The certificates of these gentlemen substantiate what has been affirmed by the agent, and establish beyond dispute, that the unfortunate occurrence in New Kent could not have originated in any want of proper precaution on his part. The committee, therefore, have exonerated him from all blame, and believe that the legislature will confirm their decision. They regard the circumstance as one of those casualties incident to the human lot, and which the most sagacious forethought or the wisest measures of precaution cannot always prevent. They would not withhold their opinion that the variolous disease was communicated from the scab sent by Dr. Peticolas, in the manner suggested by Dr. W. A. Patteson, viz: that it was a crust that had been cropped from the arm of some person in whose system small-pox was incubating when vaccinated, but in whom the varioloid was so modified by the vaccine disease that the former escaped observation.

Such cases occur, undoubtedly, and have been reported by the highest authorities, where the vaccine and varioloid disease have each run a parallel course in the same patient.

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Fortunately, until now, as far as the knowledge of the committee extends, no accident has ever happened from such coincidence. But this accident has not happened at this time alone to the vaccine agent; for among the documents referred to the committee is a letter from Dr. W. W. Parker of Richmond, in which he states that he has seen recently several cases of varioloid following vaccination with most unexceptionable matter, which had been inserted by himself. He also states that he had himself furnished the agent with virus a few weeks ago, some of which he accepted and some of which he rejected.

Now, may it not be possible that the virus of Dr. Parker and the virus of the agent had a similar birth-place, and that both were hybrid scabs, crossed with vaccinia upon varioloid, in the manner referred to by Dr. Patteson? Or, must we suppose that under high epidemic variolous influence, such as has pervaded several of the Atlantic states this winter, the insertion of the vaccine virus itself is capable of exciting true variola or varioloid? This is a question not only of great scientific but also of practical interest, and may yet give rise to something more than curious speculation.

The committee do not feel themselves justified, however, in following it any further.

But another subject, which has been brought before the committee by the documents referred to, is the abolition of the vaccine agency. Should the office be abolished? We think not. The efficacy of vaccination in preventing and in modifying small-pox, thus robbing of its horrors, the most terrific and loathsome disease that has ever yet visited man, when occurring in its unmitigated form, has been too well established in the history of the last half century to require further investigation at this day. So well recognized is this fact in the civilized world, that almost every government has embodied in its sanitary police arrangements some provision for the general diffusion of the benefits of vaccination.

In some of the despotic governments of Europe, where there are enacted strong compulsory vaccine laws, small-pox

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has been almost entirely eradicated; and in certain statistics of the kingdom of Prussia, we find that out of 490,000 persons vaccinated, only *one* has died of small-pox!

The committee agree that the agency of the state of Virginia is by no means admirably adapted to the end for which it was created. But even in its present imperfect construction, it has been a convenience and an inestimable benefit to our citizens. It has only been unable to meet the object of its creation, because the laws regulating its operations have been incomplete and ineffectual.

The committee respectfully suggest the revision and re-enactment of those laws. The difficulties besetting the agency and obstructing its usefulness have been well set forth by Dr. Peticolas. No means of perfectly preserving fresh and reliable vaccine virus longer than a few weeks has yet been devised. It has to be perpetuated only by constant vaccination of healthy subjects who have never before been subjected to the process. The amount which can thus be procured and retained, under the constant supervision of one person in a city no larger than Richmond, cannot be very great. And when in an epidemic the demand becomes extraordinary, it cannot be supplied except by the agent's procuring that which has been collected by other persons, and perhaps not always persons competent to decide on its genuineness, (which can only be known by watching the phenomena of the vaccination;) and he is thus liable to imposition. And he is more liable to imposition when the matter has to be purchased, because there is then to the seller inducement to defraud; for the matter is worth more than its weight in gold.

The committee think that these evils could partially be remedied by authorizing the agent to appoint for each corporate town of the commonwealth a subagent, to whom he could transmit virus at such times as were suitable, and to whom he could certainly look for a return, by requiring both agent and subagent to vaccinate all poor persons free of charge, and all other persons at a fixed, moderate compensation—and by enacting a law imposing a tax on every

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unvaccinated person over two years of age in any corporate town where such agency may be established. Such tax might be levied by the commissioner of the revenue and collected by the sergeant, to be paid into the treasury.

Subagents could doubtless be procured, to whom the compensation derived from vaccination fees would be sufficient pay. In this manner it is thought that the agent could better procure a constant and equable supply of vaccine matter, while at the same time this regular and systematic process of vaccination would greatly reduce the probabilities of the spread of small-pox in many communities and lessen the enormous and unequal demand for virus.

In accordance with these views, thus briefly and crudely presented, the committee submit the enclosed bill as a substitute for the 9th, 10th and 11th sections of chapter 86 of the Code of Virginia.

A Bill to amend and re-enact the 9th, 10th and 11th sections of chapter 86 of the Code of Virginia.

9. The governor shall annually appoint and contract with two fit agents, one to reside in the city of Richmond, the other in the town of Lewisburg, who shall furnish, by mail or otherwise, every physician of this state, who may apply therefor, genuine vaccine matter, with directions how to use it, free of charge.

10. The agent in the city of Richmond shall, immediately on his appointment, select for every corporate town of the commonwealth, having more than one thousand inhabitants, a subagent, to whom he shall, at least once a year, transmit a supply of genuine vaccine virus. He shall advertise once a year, and every subagent shall advertise once a year, that they will vaccinate free of charge all poor persons applying to them for vaccination—and for all other persons they may make a charge of not more than two dollars for each.

11. It shall be the duty of the commissioner of the revenue to enquire, in every such corporate town, in making out his yearly list, how many persons there may be in every family, over two years of age, who have not been vaccinated, and to impose on every such person a fine of two dollars, to be collected as other taxes, and to be paid into the treasury of the commonwealth.

12. The sum of eight hundred dollars is hereby appropriated for the payment of the agent in Richmond, and three hundred dollars for the payment of the agent in Lewisburg. The same or so much thereof as may be required, shall be paid semiannually on the order of the governor, when he shall be satisfied that the agents have faithfully complied with their contracts and duties as agents to the time of such payment.

13. All acts inconsistent with the provisions of this act are hereby repealed.

14. This act, except the 11th section, shall be in force from its passage. This section shall be in force from and after the 1st May 1857.

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ART. III.—*Practical Treatise on Stricture of the Urethra.* By J. F. REYBARD, M. D. Translated from the French. By GEO. C. STARKE, M. D. Hicksford, Virginia.

Sensibility of Strictures.

ORGANIC strictures commonly are insensible. They give no pain when the urethra is in a state of flaccidity. But for a tickling sensation, during micturition, in the canal and perinæum, they would escape the patient's notice for some time. By catheterism a pain is experienced at the point constricted, very different from that felt in the healthy portion of the canal, and which makes the patient instinctively seize the operator's hand.

Formerly, when it was believed that the tissues affected by stricture were in an inflamed condition, the pain by catheterism was ascribed to its action on the engorged urethral walls. But since it is known that the tissue of chronic coarctations has a very obscure vitality, (and the chronic are as sensible as recent strictures,) the pain is attributed to some complication—as irritable state of the mucous membrane, presence of ulcerations, &c. We admit that such exalt the sensibility of strictures; but there are other etiologic conditions, which must be recognized. When the mucous membrane is inflamed, the inflammation is nearly always behind the obstruction, as we have found by post-mortem examinations; and the sound gives pain before reaching that part. As for ulcerations, they are too rarely met with to account for the pain which is experienced in almost every case of stricture.

I think the pain is produced by compression of the healthy mucous membrane between the sound and hardened tissue, and also to stretching of this fibroid tissue. Bichat has shown that the sensibility of fibrous tissues was very much increased by submitting them to sudden, violent extension. There is another fact, that catheterism gives great pain after the mucous membrane has been destroyed by cauterization.

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Mechanism of Stricture.

Having studied the physical and pathological properties of stricture, we will notice the manner in which it constricts the urethra.

In *callous* strictures, it has been maintained that the urethra was narrowed by the internal projection of the tissue of stricture, as an incrustation of lime diminishes the calibre of a water pipe. Ducamp has given credit to this theory; and he says, with many others, that the thickness of strictures may be determined by means of *bougies à empreintes*, and that even the different kinds of stricture may be distinguished. He imagines the thickness of stricture, or thickness of the morbid tissue, to be equal to the thickness of that portion of wax displaced by the obstacle. There is nothing more fallacious than that procedure to learn the thickness of the morbid tissue; and we shall see that it has led to the most deplorable abuse of cauterization. The starting point of all these errors is the false ideas entertained as to the thickness of the tissue of stricture, and the manner in which it constricts the urethra.

Some have admitted the existence of membranous strictures, and designate them valvular and bridiform strictures, according to the disposition and extent of the membrane. Valvular strictures, they say, are formed by a thin floating membrane, adherent by its circumference to the urethral tube, pierced in its centre as the iris, and stretched across the canal perpendicular to its axis, and thus obstructing it. If the membrane is less extensive, occupying only a part of the circumference of the canal, the opening, instead of being in the centre, is lateral. The stricture is called a bride, or bridiform.

These theories would lead us to believe that the urethral walls were not altered; that the mucous membrane was not diminished in extent—or, in other words, the circumference of the duct was not constricted. It is not thus; and if the urethra had been looked upon in its ordinary form or state,

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(that of vacuity,) such an error would not have been committed. How can we admit, that in a duct with habitually closed and contiguous walls, a tissue by its internal projection could produce a stricture, or that a membrane could be developed there, and affect the dispositions spoken of?

In adopting such ideas of stricture, without thought, the urethra has been likened to a tube constantly open, or it may have been regarded only when distended by the urinary jet, or by sounds. Even then the constriction would not be caused by an engorged tissue or by a membrane united to the mucous membrane by its borders. It is a new tissue, which is substituted for the urethral walls, which does not inherit their properties, which is contracted and inextensible, and is not dilated as the other portions of the urethra.

I have never met with these valvular, bridiform strictures, and I believe they have been mistaken for the linear strictures, in which cases the urethra is constricted, as an artery is by ligature. Such a mistake is easily made; for in examining one of these strictures, if the urethra be opened on both sides of the coarctation, to within one centim. of it, a membranous fold remains, which simulates the supposed diaphragm. The same thing is observed if the urethra be injected with a coagulable material. The walls in front and behind the constriction are distended, while the constricted point is not, and the injection gives us a mould of the canal with the appearance of strangulation at the obstructed portion. From experiments, I am satisfied that this is the explanation of the membrane forming *valvular* and *bridiform* strictures. As soon as the stricture is divided, the membrane disappears, and there is no projection.

Although I deny the existence of such membranous strictures, I admit the possibility of the formation of false membranes in the urethra; but they are exceedingly rare, of small extent, and never capable of producing the mischief supposed by some, or of constituting by themselves organic strictures. And I do not believe that the coarctation will cease or the calibre of the canal be restored by simple section of that incomplete diaphragm or pretended valvula.

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In every kind of organic coarctation of the urethra, there is, I believe, a tissue of new formation, which diminishes *very* much the calibre of the canal. Whatever may be the volume, form and disposition of that tissue, the circumference of the canal is lessened by it. It differs not, whether the organic alteration is confined to the internal membrane and is limited, or involves the whole thickness of the walls and has a certain length.

The extent of the alteration of the urethral walls in organic stricture is very variable. Hunter, B. Bell and Civiale cite cases of stricture 27 millim. long; Ducamp, Ch. Bell, Boyer, 54 millim.; Choppart, Dessoult, 8 centim. Some have described stricture with callous, indurated and thick walls. I have given post-mortem examinations to show the error of such descriptions. Sometimes the tube of the stricture is uniform and straight. Again, it is unequally constricted; its walls covered with rough projections; is oblique and tortuous.

Short, linear strictures obstruct the urethra, as a ligature would if placed perpendicular to its axis. Sometimes they involve the entire circumference of the canal; then the opening of the obstacle is in the centre—or, they are confined to one side, and the orifice is more or less distant from the axis of the urethra. Although the stricture may not be more than one or two millim. long, the canal is most always somewhat constricted immediately in front and behind the coarctation.

Complete or incomplete strictures.—The degrees of constriction vary from absolute obliteration to a diminution of calibre hardly appreciable. Between these extremes, there are many intermediate, which would be difficult to classify. They may, however, be arranged under three heads:

1st. Those preventing the smallest drop of urine to pass. I think such very rarely if ever occur, though there are examples mentioned. They can only occur after repeated cauterizations or false passages.

2nd. Those which permit urine to pass in a very small stream, or by drops.

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3rd. Those which diminish the jet of urine so little that the patients are not aware of their existence. The lesion, so to speak, is latent, and is revealed suddenly by some excess. Generally, the orifice is narrower as the stricture becomes old.

The vesical orifice of long strictures is habitually smaller than the other. Sometimes it cannot be traversed by the smallest sounds. This orifice may accidentally be closed by mucus, clot of blood or gravel, and we have an obstructed, not complete stricture. Often, in front of the stricture, the walls are gradually constricted in form of a cone, its apex corresponding to the stricture; and this disposition of the parts is very favorable for treatment. The introduction of instruments is easier, and the chances of making false passages are less. This state of the canal is recognized by the aid of Ducamp's wax bougies, or by bougies à boules of different sizes. The seat of stricture may be accurately ascertained, and we know what extent to give the incision for dividing the constriction. When this disposition of the walls does not exist, and the orifice of the stricture is on the side of the urethra, catheterism is more difficult and sometimes dangerous, for the point of the bougie strikes against the stricture, forming somewhat of a cul-de-sac.

Strictures, as we have said, vary in their diameter, according to their age and etiologic conditions. Their orifices present differences which depend upon numerous circumstances difficult to appreciate, but have generally some connection with the constitution and habits of the individual, with atmospheric influences, &c. I have one remarkable example of this variation of calibre. I operated on a man in 1845 in presence of the committee appointed to examine essays on stricture for the prize d'Argenteuil. During the time that I dilated the stricture preparatory to urethrotomy, I often noticed the variation of diameter from day to day. It offered a difference of two to three millim. accordingly as the patient was examined in the morning after a good night's repose, or in the evening after the exercise of the day—vary-

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ing also according to the hygrometric and thermometric condition of the atmosphere. The stricture was generally smaller on the day after catheterism. It was larger after taking a bath. The variations in diameter are so great in some cases that they appear under the influence of the walls of the urethra. The walls contract, and the spasmodic contraction embraces the abnormal tissue, which is compressed or mechanically contracted. The tissue itself takes no part in the spasm, because we know it is not of a muscular nature.

Generally, there is but one stricture in the canal: In some instances, several have been found in the same urethra. Boyer has seen 3; Ducamp, 5; Hunter, 6; Collot, 8; Lallemand, 9. In this last observation, as the nine strictures occupied only 15 centim. of the canal's length, they can be considered as forming only one or two long coarctations. It is rare that many co-existing strictures have the same form, length and diameter. That nearest the bladder is commonly the smallest, and more sensible than those nearer the meatus, though all of them may have been developed at the same time, under the influence of the same cause. This is probably owing to the action of the urine. The degree of constriction is in connection with the seat of stricture as well as with its age.

Seat of Stricture.

There is not much difference of opinion now on this question. It is commonly stated that the region of the bulb is most frequently the seat of strictures. Ev. Home says, immediately behind the bulb. M. Lallemand, at the curvature of the urethra. Hunter, the bulbous region. A. Cooper, at the junction of the spongy and membranous portions. Brodie, anterior extremity of the membranous portion, just behind the bulb. Boyer, membranous region and commencement of the bulb. Choppart gives the same, but thinks they are rarely found behind the bulb. Ducamp

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gives the bulbous region as the most frequent seat. Larrey Delpech, the canal between the bulb and neck of the bladder. M. Dufraisse-Chassaigne pretends that the bulbous region is the exclusive seat of valvular strictures, and that they are produced by contraction of the bulbo-cavernous muscles. M. Amussat says that strictures are never found beyond the region of the bulb. M. Civiale gives the spongy portion of the bulb as the common seat of strictures.

My own observations show that strictures are situated in the bulbo-membranous region. I have found more than three-fourths of them in that portion of the canal. The next points are the fossa navicularis, the meatus, and root of the penis.

Sæmmering denies the existence of stricture in the prostatic region, but Home, Ch. Bell, A. Cooper and Lallemand have observed such cases. I have seen one instance of it, consecutive to suppuration of the prostate.

Whatever may be the seat of organic stricture, the tissue forming it is identical in its nature and properties, never partaking of the nature of the strictures which it substitutes. MM. Civiale and Leroy d'Etiolles have erred in supposing strictures are of different natures, demanding particular treatment according to the part of the urethra which they occupy.

Some strictures occupy the entire circumference of the canal, having somewhat the form of a diaphragm pierced in its centre as the iris. Others involve only a portion of the circumference, which is generally the inferior wall. Whatever may be the age of strictures, they all have the same nature, and must be treated in the same manner.

Symptoms of Stricture.

If the numerous researches on stricture have only advanced its treatment moderately, they have, at least, conducted us to a more exact knowledge of its symptoms and consecutive alterations which give such lesions so much gravity. The symptoms of organic stricture are:

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1. A discharge or oozing from the urethra.
2. Change of form and diameter of the urinary jet.
3. Frequent desires to urinate.
4. Difficulty of micturition.
5. Efforts, more or less painful, to urinate.
6. Retention of urine.
7. Incontinence of urine.
8. Pain during micturition.
9. Modifications of the urine.
10. Troubles of the genital functions.

In the chapter on diagnosis we will give the symptoms obtained by examination of the urethra.

A *slight discharge*, having no contagious characters, commonly accompanies organic strictures during the first period of their formation. This catarrhal secretion, often not observed in the day, accumulates at night in the canal, appears in the morning at the meatus, as a small drop more or less opaque, and vulgarly known as *goutte militaire*. But very often this liquid, secreted at the stricture and behind it, does not reach the meatus. It is arrested at the obstacle, grows hard, and is expelled from the canal by the jet of urine. This stopper, or thread of a greyish white color, may be obtained by receiving the urine, first passed in the morning, in a glass half full of water.

The first functional alterations which attract the patient's notice are *modifications* of the jet of urine. These changes relate to the diameter, rapidity and form of the stream.

The diameter of the stream diminishes insensibly but progressively. From the size of a goose quill, it becomes about the size of a crow's quill, and may remain at that degree of diminution without inconvenience, if the patient is sober and leads a regular life. Unhappily, these favorable conditions are rare. More frequently, the stream of urine continues to get smaller, and, finally, is no larger than a thread. After that period, the urine is passed by drops. The diameter of the jet may vary from day to day, according to the individual's habits.

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The *rapidity* of the jet diminishes pari passu with the volume of the stream, and the duration of micturition varies according to the degree of diminution. The urine from a healthy man is expelled in a curved stream about the distance of one metre, and requires a few seconds only for its emission. In stricture, the length of the jet becomes less and less, its curve diminishes gradually, and after a time is perpendicular; *le malade pissera sur ses souliers.* The duration of the act is from one to five minutes, and may be longer.

When the stricture attains this degree, the patients are sad, morose and taciturn. They go alone to unfrequented places to urinate, so that no one may witness their infirmity.

In health, towards the end of micturition, the perinæal muscles, by two or three abrupt contractions, empty the urethra of its contents; but when the canal is constricted, five, six and even more of such contractions are requisite to expel the last drops of urine.

The *form* of the jet generally becomes flattened. Then it is divided into two diverging streams, or very many smaller streams, which twist around each other, having somewhat the appearance of a corkscrew. At last, the stream is interrupted, the urine is voided slowly and by drops.

In the commencement of the stricture, the person, without observing it particularly, has desires to evacuate the bladder more frequently than when in health. Being occupied during the day, he does not satisfy these desires; but at night they are imperious—his sleep is disturbed, and he is compelled to discharge the contents of the bladder. There are some patients obliged to urinate every two hours during the night. This is the case when there is incomplete retention—and the reason is this: The principal agents in the expulsion of urine are the muscular layers of the bladder. These fibres, crossing each other in various directions, can empty, by their contraction, completely the bladder of its contents, if there is no obstacle to the stream of urine; but when there is a stricture, the jet is impeded, and the

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muscular fibres are exhausted before the urine is entirely expelled. The walls of the bladder are fatigued and relaxed. The expulsive force ceasing, the patient thinks he has emptied the bladder, when in fact there remains more or less urine. It results from this incomplete evacuation that the desires to urinate are more frequent, and are at shorter intervals. They become incessant even when the urine, arrested in its passage by an advanced coarctation, accumulates in the bladder, and is discharged only by drops.

It is generally said that the difficulty in urinating increases with the degree of constriction. As the stricture becomes more narrow, the jet meets with more resistance in passing it, and is finally interrupted, so that the urine is voided by drops, or not at all. In fact, it is thus in the majority of cases, but exceptions to it are not rare.

Very often we find patients who urinate easily and have quite a large jet, although their strictures will only admit a very small bougie. There are other patients who urinate with difficulty, have a very small jet, urine even escaping by drops, and yet sounds of a large volume may be introduced. What is more astonishing in such cases is, that the dysuria sometimes increases to complete retention, and fistulæ are produced.

The first case is explained thus: the patient urinates easily, and the diameter of the jet at the meatus is greater than the diameter of the constriction, because the bladder is hypertrophied and capable of more energetic contraction, expelling the urine more forcibly. If the stream at the stricture is smaller, it passes more rapidly, and that compensates for the diminution in volume.

In the second instance, the difficulty of micturition, with the volume of the jet inferior to the orifice of the stricture, is explained in an opposite manner; that it is caused by fatigue, inertia and paralysis produced by the excessive accumulation of urine in the bladder. But in examining such cases, we do not find the bladder thin and relaxed, as is supposed. Its walls are thickened as if hypertrophied, and

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we must give another explanation for it. We think the facts are in direct connection with our ideas of the tissue of dilatable strictures. We have spoken of the extensibility of the walls of some coarctations; that they are so elastic as to permit the passage of sounds with very little resistance, but the urinary jet could not separate them with the same facility.

The *efforts to urinate* in health are so slight that we are hardly conscious of them; but when an obstacle opposes the current of urine, the efforts are more marked, and vary according to the degree of constriction. In the beginning, the person calls into action the abdominal and perinæal muscles. If he overcomes somewhat the obstacle, he seeks attitudes which favor his efforts. To give fixedness to the body, the hands are placed upon the knees or surrounding objects, and the prolonged efforts produce congestion of the superior portion of the body.

When the degree of constriction is a little more advanced, micturition can only be accomplished in the attitude of defecation; which position is very favorable to the production of hernia; and the repeated efforts favor also cerebral congestion, epistaxis, hemoptysis, &c.

Retention of urine may be partial or complete. It may be called partial with those who can only urinate by drops, after powerful efforts. Complete retention is much more serious. It is the most formidable accident which complicates strictures, not only from the sufferings which accompany it, but because the life of the person is in danger. In considering such a fearful event, can we understand the course of practitioners who do not haste to prevent it in attacking the cause by a curative treatment? This accident is to be feared in every stage of stricture. By it a stricture, before unknown, not suspected, is revealed. The person, after some excess, desires to urinate; and to satisfy that desire, makes useless efforts. After five or ten minutes, the desire leaves him, but soon returns—is more pressing—more imperious. The patient, endeavoring to get relief, assumes

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the attitudes which are favorable, and calls into action all the expulsive forces of micturition. The tortures are extreme and the danger great. Finally, the surgeon resorts to the most prompt and energetic means for overcoming the difficulty. Otherwise, the bladder or urethra is ruptured, urine escapes, and is infiltrated into the cellular tissue of the perinæum and scrotum—sometimes a part of the urine is absorbed, taken into the circulation, and the accidents of the *fièvre urineuse* arise.

Incontinence is false and true. When the person has finished urinating, there is an involuntary discharge of urine by drops. It is due to a certain quantity of urine which remained in the enlarged canal behind the stricture, and is expelled a few instants after the last contractions of the bladder, by its own weight. This is false incontinence.

If the urethra behind the stricture is more enlarged, and the enlargement extends to the neck of the bladder, the neck becomes dilated, and is no longer capable of retaining the urine in the bladder. It is the stricture and not the neck of the bladder, which performs that office. Urine escapes, involuntarily, by drops, and its discharge is constant, being increased in quantity by cough, sneezing, walking, and any exercise producing contraction of the abdominal muscles. This is complete incontinence.

It is only at an advanced period of the lesion that the patients complain of *pain* in urination. The pain is not constant, and varies according to the temperament of individuals. Sometimes it is a pricking sensation, itching or tickling. Again, it is a burning so acute that the patients dread the act of micturition, and frequently stop during the emission. Often, after urinating, the itching sensation is prolonged, causing erections and new desires to evacuate the bladder. Commonly there remains a feeling of malaise and weight in the perinæum. This dull, heavy pain is attributed to dilatation of the urethra behind the stricture, to inflammation, and ulceration of the mucous membrane of the urethra.

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Although the stricture may be situated in the bulbous region, the pain very frequently appears localized, and seated at the meatus or fossa navicularis—the *rendezvous* of all the sympathetic pains of the genito-urinary organs.

If the person is nervous, and the urethra behind the stricture is in an irritable condition, the sphincter of the neck contracts and relaxes alternately, to let pass but a small quantity of urine to prevent the pain, by dilatation of the stricture which sudden evacuation produces. Such instinctive contractions of the neck of the bladder may increase so as to oppose completely the emission of urine.

This spasm depends upon the organic coarctation. It is an epiphenomenon which ceases when the obstacle to the jet of urine disappears. Without doubt, this is the kind of stricture which M. Civiale says produces neuralgia of the neck of the bladder. I have seen, says he, "many examples, and I could not admit, without hesitation, the influence of a cause which at first appeared to have no bearing; but I only incised the meatus, and introduced small bougies to prevent the immediate healing of the little wound, and all the accidents disappeared. Such instances are so numerous that my doubts are dissipated, and the result is so constant that I announce it as certain."

Modifications of the urine.—In respect to *quantity*, the diminution is only apparent, for the frequency of the emissions compensates for the small quantity voided each time. Certain conditions can, however, increase or diminish the quantity of urine. Some patients, fearing the pain caused by micturition, abstain as much as possible from drinks and liquid aliments. Deceptive precaution—for if the urine is less abundant, the desires to urinate are not less frequent, and the urine being more concentrated, charged with saline principles, irritates more the parts bathed by it, and particularly the stricture. On the contrary, other persons, from experience, take freely of drinks and diuretics. The urine is more abundant, but less irritating.

The *quality* of the urine is not notably altered, unless

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stricture, old and advanced, has brought on some complication of the bladder, prostate or kidneys. If it remains long in the bladder, it becomes ammoniacal, less watery, and is much troubled. It sometimes contains pus, blood, semen, and very often a glairy, tenacious matter, which Ev. Home considers an indication of prostatic lesion.

Disturbance of the genital functions.—With some persons there is no pain during coitus. With others it is supportable. With another class, it is so acute that they abstain from the act. The pain is much greater than that experienced in urinating, and is accounted for, generally, by the sudden distension of the canal behind the stricture. My own opinion is, that the pain is provoked by abrupt dilatation of the stricture itself.

Coarctations in maintaining the canal in a state of superexcitation, produce frequent erections, often painful, and increase the desire for sexual intercourse. When the stricture is far advanced, the emission of semen is incomplete; a part or the whole is retained behind the obstacle, and afterwards oozes away, without voluptuous sensation, or it flows back into the bladder, and is expelled with the urine.

When the inflammation extends to the genital organs, the nature and quantity of the semen may be so altered that the procreative faculty is lessened. In some cases, there is incontinence of semen, whether there is or is not dilatation of the ejaculatory ducts. This variety of seminal emissions is described well by M. Lallemand.

Although the seminal emission may enlarge momentarily the orifice of stricture, and micturition may be easier for a day after, experience proves that such excesses nearly always increase the stricture, by the pain and irritation consecutive to the sudden dilatation from the passage of semen.

Contrary to what has been said before, urethral coarctations may become the cause of impotence, from the fact that erection may be difficult or impossible.

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ART. IV.—*An Unusual Phenomenon.* By L. B. ANDERSON, M. D. of Verdon, Hanover County, Va.

DURING the cold weather of the last winter, many *pine trees* have withered and died. Nor has this fate befallen the young and tender trees and shrubs only, but many of the oldest and largest in the forests. They present the appearance of having been subjected to the impression of intense heat, and have become withered. The leaves are all red and brittle, and the bark is loose and exsiccated. I find many other (especially fruit) trees have also been killed. This phenomenon has excited in my mind many conjectures and curious enquiries.

I have been taught to believe, that on the approach of winter the sap leaves the branches and bodies of trees, and descends into the roots, there to remain until the warmth of spring shall again bid it rise. Knowing that nutrition and growth are effected by the sap, I had supposed that on the retrocession of that fluid, those processes ceased altogether, and the tree remained in a *quiescent* state, similar to a cold blooded animal, which becomes torpid on the approach of winter, thus to remain till congenial warmth arouses it. But we find, that from the influence of intense cold, the bark of trees becomes loose, the wood turns dark, the leaves wither, and vitality is entirely destroyed. The question arises, if the circulation is suspended and all nutrition thereby necessarily arrested, how is it that intense cold can cause a total destruction of all vitality?

If the circulation were in active progress, and all the vital functions were in healthful exercise, we can readily imagine that a congelation of the sap would destroy nutrition, arrest vitality and produce dissolution. But when all these processes are suspended by purely natural causes, it seems rather remarkable that any degree of cold could produce death *immediately.* We would not have been surprised to find, that from the effect of intense cold, the whole organization had been so much affected that when the sap should be called out by returning spring, it would find its channels

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so much altered or disorganized by a *purely mechanical** power, as to arrest its circulation, and thus produce death. But to appreciate our difficulty, it must be borne in mind that *dissolution* resulted *immediately*, or while still under the influence of intense cold.

Two questions suggest themselves at this point: One is, can the circulation be arrested during winter in vegetables and trees? And the other, if it be arrested, is not nutrition still carried on by some other process? Both of these questions, however, suggest difficulties not easily surmounted. If the circulation is not arrested, and nutrition is still carried on, why is it that trees do not increase in size during winter as they do in summer? And if the circulation is arrested, in what other way can nutrition be effected?

We submit:

1st. That the circulation in the vegetable kingdom is arrested, during winter, in the *superficial*, but is still carried on in the *deep-seated* vessels.

2nd. That nutrition is effected by a process of *transpiration* from those *internal* vessels, to just such an extent as will maintain an equilibrium between *decay* and *growth*, and thus sustain vitality.

3rd. That growth only begins, when the circulation is called to the surface by the congenial warmth of returning spring.

4th. That animals, which lie *torpid* during winter, have their vitality sustained in the same manner, and consequently have a circulation of the vital fluid through the vessels of deep-seated organs and tissues, while it is suspended in the cuticular and more superficial vessels—though we presume there would be *some obstacle* in the way of *such a circulation as this!*

5th. Are all of these conjectures right? If not, which are? And if none are, will you or some one of your scientific contributors give a solution of this (to me) exceedingly interesting problem?

* The word "physical" might have been used with more propriety, in contradistinction to "vital."

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ART. V.—*On an Easy Mode of Constructing Bougies.* By P. H. CABELL, M. D. Selma, Ala.

I WISH to call the attention of the medical profession to an easy and rapid mode of constructing bougies, which I think presents some advantages, both as to the qualities possessed, and the facility and cheapness with which they may be made. Reflecting upon the advantages the bougies made of elm bark possessed, from the ease with which they are introduced, and the expansion they undergo while in the urethra; and then thinking of the danger of breaking, the difficulty of treating deep-seated strictures, and the grave accidents which sometimes occur, I determined to seek some substitute, which would possess its good qualities, and be free from all risk.

The substance I finally selected was untanned cow hide; which may be obtained sometimes of great thickness. It is first to be well soaked in water, then cut into strips of suitable length and width, and tacked by the extremities over a block of wood of the proper curve. When wished straight, no form is necessary, they being merely stretched on a plane surface till dry. When dry, they are found very tough, unyielding, and of sufficient elasticity. They may be brought to the proper size by the knife, rasp, sand paper, &c, and will be found to have a fine polish, which allows them to be introduced with ease; they are much more rigid than either the wax or gum instruments, but they are sufficiently yielding to be perfectly safe unless great violence is used, and even then I do not conceive that there could be much if any risk of making a false passage.

There are two ways of preparing them for use—one by oiling as usual, and the other by dipping for a few moments in warm water. The point may be previously well softened by a longer immersion in water. It thus becomes almost jelly-like, and glides easily and painlessly along the urethra. If the surgeon does not wish to avail himself of their ex-

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pansiveness in dilating the stricture, he may cover them with a solution of gutta percha, in chloroform, which will protect them from the action of the urethral mucus, and render them beautifully polished.

I do not claim anything for these bougies but the ease and cheapness with which they can be made, and the entire freedom from danger, and as giving to the country surgeon the means of making his own instruments, suitable to the various contingencies of his cases.

ART. VI.—*Remarkable Consequences of Vaccination during the Recent Small-Pox Epidemic.* By WILLIAM W. PARKER, M. D. Richmond, Virginia.

AT the suggestion of several friends, I have to ask of you the publication of some facts and remarks upon vaccination. On ordinary occasions, the request would be unwise. Of nothing was I more satisfied until recently, than of the certainty and completeness of our knowledge of everything practical that appertained to vaccination. In a practice of six years, I had not encountered the smallest difficulty in its employment, nor had I heard of any untoward results in the practice of other physicians. The text books had given no word of caution; nor had my practical teachers sounded an alarm. Judge then of my astonishment when certain facts which I will now proceed briefly to relate, came to my ears. In order to prevent confusion and also for the purpose of aiding those of your readers who may be blessed with acuteness of perception and strong powers of analysis, I shall give you a plain history of my vaccination practice, with a tabular statement from November last to the 7th instant. I frankly confess it contains several enigmas beyond my ken.

Shortly after my resumption of practice, I was called to see a case of small-pox. Not knowing anything of the presence of the disease in the city, I did not make my diagnosis until the second day of the eruption. Four persons occu-

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pied the small room in which the patient lay, viz. her husband and two children—the two latter, like the mother, were entirely unprotected. The father had been vaccinated. Having no virus, I procured some immediately from a medical friend, and vaccinated the father and children. The vaccine disease ran its usual course in *all three.* I saw these persons daily in my visits to the mother, who had a bad case of distinct small-pox, from which she recovered. I reassured myself on yesterday, by calling at the house, of the fact, that the children had no eruption.

With this ammunition, I began the campaign. I did not, however, use the scab from the father's arm, and only vaccinated one or two persons with *one* of the scabs from the children, because it was not of the characteristic mahogany color, but of a greenish yellow. The other healthy scab I used, and also gave part of it to a medical friend, who met with no untoward result from its use. The vaccination of the above mentioned three persons was performed on the 3d of November, as will be seen by reference to the annexed table, which comprises all the vaccinations of which I made any record within the periods above mentioned. About three or four weeks afterwards, I vaccinated two children with virus procured by the mother from the vaccine agent. I had previously vaccinated them with the virus just mentioned, but it failed, and having no other, I requested the mother to make application to the agent, who supplied her so liberally that I had some of it left for my own use.

I continued to vaccinate up to case No. 13, (see table) without the occurrence of anything unusual. The vaccination in this case will be seen to have been practiced on the 18th of January. On the 26th of January I was sent for to see the infant, who had two convulsions. I saw it about six hours after the last, and found it almost well, needing nothing but a purgative. I examined its arm and also that of the older child, a boy of five or six years, and found both presenting the characteristic eruption (on the arm.) The family living out of the city, and the weather being very

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severe, I requested that I should be sent for if any further services were needed. Some days after this visit a medical friend, to whom I had on two occasions given virus, called on me, saying, that the matter I had last given him had produced varioloid in one or two cases, and that others, whom he vaccinated with it a few days before, he feared would have the same result. He also informed me that he had learned that the children (No. 13) above mentioned had varioloid. I denied it upon the authority of my last visit and the request made to the father. This friend's cases turning out as he feared and hearing similar reports of case No. 13, I went to the house of the father about a fortnight after my last visit (26th) and found the children perfectly well and without the slightest mark about them. The mother informed me that she had noticed an eruption upon their body and legs, especially of the oldest, but nowhere else. From her description of its character and the shortness of its stay, I can scarcely conceive that these were cases of varioloid eruption. Indeed, I regard the appearance of the eruption upon the *face* as almost invariable. I learned afterwards that the mother of these children had varioloid, supposed to have been taken from her children. I did not attend her.

About the 1st of February I gave some virus to a medical friend from the country, observing to him at the same time that I got it from the arm of a very healthy child, whom I vaccinated myself, and that it was very good. I am not now certain to what child I alluded. This friend informed me that he vaccinated 16 persons with it, and all had mild inoculated small-pox or varioloid—one case excepted, which was very severe. There was at the time a case of small-pox two miles off, taken in the natural way. From these 16 patients 4 unprotected persons took the disease, but all recovered. About the same time I supplied two medical friends of this city, (one several times,) and also sent virus twice to Chesterfield county, from none of which I received any but a favorable account. In the mean time I continued to vaccinate almost daily, as will be seen from the table,

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from the same samples of virus which I gave to others; for in no case up to that period had I given to any one an *entire* scab.

On the 24th of January I vaccinated a lady and her child from Charles City, on a visit to a relative. Neither had been previously vaccinated. I requested the husband to remember from whence I got the virus, informing him of the name of the child (No. 7) from whose arm it came. I had become cautious, very naturally. The father, whom I revaccinated, remained in the city. His arm was quite inflamed, and presented so much of the peculiar appearance of the vaccine disease in the part, (the arm,) that I took the scab with the intention of trying it on some one else, but never did so. I have learned recently from the mother in person, and also from the family physician, that she (the mother) had considerable pustular eruption on the face and extremities, and doubtless communicated *small-pox* to her father's family, all of whom were unprotected. Her child, strange to say, vaccinated at the same time and with the same matter, *had no eruption whatever*, nor was the vaccine disease unusually severe.

With this exception, it was not until I had vaccinated upwards of 100 persons that I either saw or knew of but one case of varioloid resulting from my own vaccination. This was case No. 45 in the table. This was a lady who was also on a visit to Richmond, and who had been vaccinated five times without success. I vaccinated the day before, in the same house, four children, but she was rather averse to the operation, thinking it useless. I informed her that the vaccination would almost certainly take—that I had never seen such an aptitude in the system for the vaccine disease, and that very many if not most of the cases of *revaccination* were successful. She was vaccinated on the 8th of February, with what virus I do not remember, but imagine it was with the same with which I had the previous day vaccinated her sister's four children. While, however, the vaccine disease

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ran its wonted course in them, the aunt had to take to her bed on the 8th day, and on the 11th the varioloid eruption (or the inoculated small-pox, if you prefer it,) made its appearance. There were some dozen pustules upon the face, which filled (with pus of course) and then dried up. From this day I resolved to keep a record of every person vaccinated, and of the virus employed; and to this resolution I have constantly adhered. Now see the result:

On the 18th of January I vaccinated a girl of 8 years and one of about 18, (case No. 15.) I do not know with what virus. This was *before* I came to the resolution just mentioned. I saw these patients on the 8th day. The vaccinations had taken beautifully. The younger was not at all sick, and the elder had but little constitutional disturbance. On the 14*th of February* I called for the scabs. I wanted but one, and took that from the child. The aunt as well as the girl informed me most emphatically that the child had not a pimple of any sort from head to foot—that she had not been sick, and had been always at her place at the table, almost with her usual appetite. I have been reassured of these important facts by a recent visit. I enclosed this scab in a piece of paper, making on the spot a memorandum of the facts in the case. The child being a perfect picture of health, I valued the scab the more.

On the 22nd February I vaccinated a mother and three children with this virus. The mother had been vaccinated some years before; the children only about *a fortnight.* The father performed the operation in the case of the children with matter gotten from a neighbor, but not thinking it good, he applied to me to revaccinate them. On the arm of one of the children there still remained two greenish yellow crusts. These children were made very sick by the revaccination. The youngest, a boy of four years, was unable to walk for four or five days from the pustulations on the feet. There was much eruption also on the face and nates. This was a case of inoculated small-pox beyond doubt. The two sisters had but a slight varioloid eruption on the scalp

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and face, which dried up in a few days. I took a piece of the old crust from one of these children and vaccinated a patient with it, inserting at the same time other virus. The first mentioned inflamed the part a little and formed a small scab, but seemed throttled by the other virus at work in its neighborhood.

On the 23rd February, I vaccinated with virus from No. 15, three children, aged respectively 6 and 4 years and 16 months. The two oldest had been vaccinated. All these were made sick. The oldest had varioloid, though mildly; the second had *no eruption whatever;* but the infant of 16 months was seized with convulsions on the eighth day, which continued during the eigthth, ninth and tenth with great violence, when the eruption made its appearance, and which proved to be confluent small-pox. This child was ill till the eleventh day of the eruption, when the scabs formed rapidly and fell off. The secondary fever was very slight.

On the 25th I inserted some of the same virus into the arm of a child three months old. This case, though not preceded by convulsions, was very severe. The vaccination at the point to which the virus was applied seemed to have had, as in the other case, much the start of the eruption elsewhere, though it did not appear much to modify it. On the legs and arms the pustulation proceeded well. The eruption on the head and face, which threatened to be confluent, did not pustulate, but began rapidly to dry on the tenth day. Up to this time, the little sufferer nursed heartily and seemed in a fair way to overcome its powerful foe, but on the eleventh day its respiration became difficult, and stimulation, which had been commenced the day before in anticipation, was pushed with vigor. The drying and scabbing went on rapidly, and the swelling of the face subsided, but the little sufferer, sad to relate, died on the twelfth day of the eruption, from exhaustion.

On the 23rd February, and with the same virus, I revaccinated two members of my own family, both of whom had mild varioloid. Of the six other persons to whom this virus was applied, three had hever been vaccinated. Two of these

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had varioloid. The third, an infant, had the vaccine disease simply. The other three of the above mentioned six had been vaccinated, and received no damage from this poisonous virus. There is no doubt whatever about this scab. (No. 15.) I have a part of it now in the paper in which I originally enclosed it. It may be well to state, that about a month (24 days) after this child was vaccinated, her sister, with whom she lodged, and who had not been out of the house since the 25th of December, was seized with small-pox. She refused to be vaccinated at the time her sister submitted to the operation. The case was of the distinct variety and quite mild. It occurred in a large boarding-house, and it was necessary to remove the patient to the hospital.

For the purpose of giving a full history of my connection directly and indirectly with the unfortunate results of vaccination here and elsewhere in the state, and also for the purpose of defending others who may have been censured, I wish to state that I sent, early in February, vaccine matter to my friend Dr. Washington, who resides near Fredericksburg, marked "from the arm of Rev. J. E. W.'s child, two years old and healthy." I requested him to note the result, as we were having here some strange disclosures from vaccination. The note came sooner than I expected. In a letter dated Clifton, March 5th, the Dr. informed me that with this virus he "vaccinated on the 19th, 20th, 21st and 24th of February some 30 or 40 persons, and in every case where the persons had not been protected by previous vaccination they had varioloid." There has been no report of another sample of virus sent him. Some time in February, (about the 15th, I think,) I gave to a friend, a large farmer, who had come to Richmond for the purpose, pieces of two scabs, one from the arm of the oldest son of Rev. J. E. W. No. 16, and the other from Mr. S.'s infant, No. 12, requesting him to keep them separate and let me know the result. The first injunction was unfortunately disregarded, and he informed me on the day before the meeting of the late Democratic convention, that he *mixed* the virus together and vac-

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cinated about 40 persons, and that all "broke out;" that never having seen vaccination performed before, he had supposed this result to be usual, but that since coming to the city he had been alarmed by being told that he had inoculated his family and servants. He therefore resolved to return home next morning both for the purpose of relieving his apprehensions of the result, and also to vaccinate his brother's servants who lived on an adjacent farm.

I immediately repaired to a family where I had some three weeks before vaccinated three healthy children, and procured three scabs. On enquiry of the mother whether her children had any "breaking out," at any time of the vaccine disease, she answered, "not a particle." I gave to this friend, who resides in Caroline, the best looking scab of the three, and I have since heard from him that the persons vaccinated with it had an eruption. A scab from one of the other children I have used several times in my own practice, but with no such result.

It seems, then, in regard to the children of Rev. Mr. W. that many persons were inoculated from each of them. To show that neither of these children were suspected of having varioloid, the father came to my office some weeks after his children had been vaccinated, and wished me to insert some matter into his arm. He had been previously vaccinated, of course. I had several scabs, and told him he might take his choice. On learning that I had one from his own child's arm, he said, "give me some of my own blood." This revaccination did not take. Since the receipt of Dr. Washington's letter, I called to see this gentleman's wife, and was told that her youngest child had some pimples between the eyes and on the back, but that she thought it unnecessary to mention it. That these were mere *pimples* is clear to my mind, from the fact that having to attend the oldest child, a delicate, nervous boy of some eight years old, for four or five days for fever, the result of the vaccine disease, I noticed the baby every day and fondled it, without observing any eruption. This child needed no prescription.

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The oldest boy was quite sick and had some eruption on the legs, not such as I have seen or heard described as varioloid pustules, but simple, minute papulæ, which I have often known to occur in the progress of simple fever.

In examining the annexed table, I find that of the 151 persons vaccinated, 11 had varioloid and two confluent small-pox, as the result. One of these patients, who had slight varioloid from revaccination, was nearer her accouchement than I had supposed; and before she had entirely recovered from the effects of the *varioloid*, she was delivered of an apparently healthy child. This infant was vaccinated on the third day. The vaccine disease ran its usual course, but on the 14th day after the insertion of the virus, a mild varioloid made its appearance. This disease was most probably taken through the system of the mother.

All of the above mentioned cases of varioloid, with two exceptions, were produced by virus taken from case No. 15—one of the excepted cases most probably from No. 7. Of the virus used in the other case I know nothing, except that I cropped it myself from *some child's* arm.

Since resolving to make this publication, I have visited and enquired into the results of most of the persons vaccinated, where there was any doubt. I have found that besides the thirteen cases above mentioned, *seven* others had eruptions, but not sufficient to attract the attention of the physician at the time, nor mention on the part of the nurse.

I am quite sure that I never used *three* of the seven scabs procured from these patients. The remaining four I either used or gave away; though as stated before, I rarely, if ever, gave away an entire scab, except in the case of the country friend above mentioned. To one physician in this city I gave virus as often as five times, yet in no case in which it was used did any unusual results occur.

The length of the foregoing communication, together with other reasons, preclude a lengthened commentary at this time on the curious facts before mentioned. One very important conclusion may be however safely drawn, viz: that

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there is *no means* of ascertaining with absolute certainty whether or not any given scab is *genuine.* That there is in this city at present a very unusual small-pox epidemic influence abroad is undoubted, but it cannot with any reason be supposed that almost every person vaccinated with scab No. 15 was *peculiarly* under this influence. That a true vaccine scab may be so modified by atmospheric influences as to produce small-pox in the unprotected, has been neither proved nor disproved, so far as I know.

In conclusion, I wish to state that I got no virus at any time from the state agent, except indirectly as above mentioned. On the contrary, I gave him virus on two occasions. Indeed, I had generally an abundant supply on hand, and used none except that cropped from the arms of apparently healthy children.

Statistics of Vaccinations from November 3, 1855, to March 7, 1856.

Number.	DATE.	No. of Persons.	White.	Black.	Minor.	Adult.	Vaccination.	Revaccination.	REMARKS.
	1855.								
1	Nov. 3	3	3	–	2	1	2	1	Small-pox in house.*
2	16	2	2	–	2	–	2		
3	26	1	1	–	1	–	1		
4	26	1	1	–	1	–	1		
5	Dec. 22	1	1	–	–	1	–	1	
6	23	6	6	–	3	3	6	–	Reported eruption on two. Scabs not used. Small-pox in house.†
7	24	1	1	–	1	–	1	–	Reported eruption. Used the scab in 8 or 10 cases.
	1856.								
8	Jan. 2	1	1	–	–	1	–	1	
9	3	2	2	–	1	1	1	1	
10	13	2	–	2	2	–	2		
11	14	3	–	3	3	–	3		
12	18	1	1	–	1	–	1		
13	18	2	2	–	2	–	2	–	Reported eruption. Scab not used.
14	18	1	–	1	–	1	1		
15	18	2	2	–	1	1	2	–	No eruption whatever. Scab produced small-pox.‡
16	19	2	2	–	2	–	2	–	Reported eruption. Scabs used.
17	20	2	–	2	2	–	2		
18	20	2	2	–	2	–	2		
19	24	1	–	1	–	1	1		
20	24	3	3	–	1	2	2	1	One had varioloid. Virus used from No. 7.
21	23	1	1	–	1	–	1	–	Reported eruption. Scab used in 10 to 15 cases.
22	24	1	1	–	1	–	–	1	
23	27	5	–	5	5	–	5		
24	29	2	–	2	2	–	2		
25	29	1	1	–	–	1	–	1	
26	29	1	–	1	1	–	1		

* This is the matter I gave to a medical friend, who used it without any ill consequences.

† These were either vaccinated with the scab from No. 1, or the matter derived from the agent.

‡ There was *certainly* no eruption here, yet this scab in 11 cases produced varioloid and confluent diseases in two instances.

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Number.	DATE.	No. of Persons.	White.	Black.	Minor.	Adult.	Vaccination.	Revaccination.	REMARKS.
	1856.								
27	Jan. 29	1	–	1	–	1	1		
28	28	1	–	1	–	1	1		
29	29	1	–	1	–	1	1		
30	28	1	1	–	–	1	1		
31	30	4	–	4	4	–	4		
32	29	2	–	2	2	–	2		
33	Feb. 2	2	2	–	2	–	1	1	
34	1	1	1	–	–	1	1		
35	2	1	–	1	1	–	1		
36	2	5	5	–	–	5	–	5	
37	4	4	2	2	4	–	2	2	
38	4	2	2	–	2	–	2		
39	4	5	2	3	–	5	1	4	
40	4	5	4	1	4	1	4	1	
41	5	6	6	–	3	3	3	3	
42	5	1	1	–	–	1	–	1	
43	5	4	4	–	4	–	4		
44	9	7	4	3	4	3	3	4	
45	8	3	3	–	2	1	3	–	One had varioloid. Had been vaccinated 5 times.
46	8	1	1	–	–	1	1		
47	8	1	1	–	–	1		1	
48	13	3	2	1	1	2	1	2	
49	16	1	1	–	–	1	–	1	
50	16	2	1	1	1	1	1	1	
51	14	1	1	–	1	–	1		
52	15	1	1	–	–	1	1		
53	16	1	1	–	1	–	1		
54	17	2	2	–	–	2	–	2	
55	18	2	2	–	–	2	–	2	
56	23	1	1	–	1	1	–	–	Had varioloid.*
57	23	3	3	–	3	–	1	2	One had varioloid. One confluent small-pox.
58	23	2	2	–	–	2	–	2	One had varioloid. Revaccinated.
59	22	4	4	–	3	1	4		Three had varioloid. Do.
60	24	2	2	–	2	–	2		One had varioloid.
61	26	1	1	–	–	1	–	1	Had varioloid.
62	22	1	–	1	–	1	1		Had varioloid.
63	25	1	1	–	1	–	1		Had confluent small-pox. Died.
64	15	1	–	1	–	1	1		
65	Mar. 1	1	–	1	–	1	1		
66	1	1	–	1	1	–	1		
67	1	1	1	–	1	–	–	1	
68	1	1	1	–	–	1	–	1	
69	5	1	1	–	–	1	–	1	
70	5	1	1	–	1	–	–	1	
71	7	2	2	–	2	–	2		
72	6	3	3	–	3	–	3		
73	7	3	3	–	1	2	3		

* I do not confine the use of the word "varioloid" to persons *previously vaccinated.*

☞ The cases from 56 to 63 inclusive, were vaccined from No. 15.

☞ Of the 151 persons vaccinated, it is seen that 92 were adults. Four-fifths of them were revaccinations, yet more than half of them "took," and in all there was much inflammation produced

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LECTURES AND SELECTIONS.

I. *On Cauterization by Galvanism.* Read before the Boston Society for Medical Observation. By ALGERNON COOLIDGE, M. D.*

Within the last twenty years a new method of applying electricity to medical purposes has been tried, and promises to become of greater practical utility than any other. This is the method of employing a wire, heated by galvanism to a red or white heat, instead of the knife, for some operations, and always instead of the old means of applying the actual cautery.

The first mention we have been able to find of this cauterization by galvanism, is in a passage of Becquerel's "Treatise on Electricity."† He says, "Dr. Fabré Palaprat has found in electricity a very simple way of applying instantaneously a moxa to the deepest seated parts of the body, without producing any appreciable lesion except at the point where it is applied. For this purpose a platinum needle is introduced into the affected part, and is put in communication with one of the poles of a voltaic pile, composed of elements with large surfaces, capable of producing powerful thermo-electrical effects; while the other pole, by means of a metallic plate, is in contact with a neighboring part of the body. The needle immediately becomes incandescent and burns the adjacent tissues, producing a strong pain but of short duration. Some days after, inflammation similar to that produced by a moxa sets in, and is followed

* We had commenced a translation from the French version of a pamphlet, Die Galvanocaustik, published in 1854 by Dr. A. T. Middeldorpf, Professor of Surgery in the University of Breslau, and abridged for the Archives Générales de Médecine, August, October and December 1855, by Dr. Axenfeld, when we observed the very excellent paper on the subject, published in the Boston Medical and Surgical Journal, November 1855, which we transfer to our pages, as conveying, in a comprehensive form, all the information which we could have obtained from the more elaborate memoir of the Archives Générales. In Dr. Coolidge's paper, the principal instruments employed by Professor Middeldorpf are represented by figures; and the editors of the Boston Journal, with their usual kindness and courtesy, have allowed these wood cuts to be used in connection with the present reprint. The methods of Dr. Middeldorpf are thus made much more intelligible.—*Editor.*

† Traité de l'Electricité. Paris. 1836. V. 4, p. 306.

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by a scar which separates in the form of a quill." There is some mistake in this statement of Becquerel. The means he gives, as used by Fabré Palaprat, cannot produce the desired effect.

We must date, therefore, the first practical use of cautery by galvanism no further back than 1843.

Heider, in Vienna, acting upon a suggestion made to him two years previously, by Prof. Steinheil, of Munich, employed the galvanic cautery for the destruction of the dental pulp.

In 1844, fourteen months after the experiments of Heider, Louyet, in the Archives de la Médecine Belge, recommends the same method for the same purpose. He suggests combining the killing of the nerve and the filling of the tooth, by means of a melted globule of metal dropped into it.

In 1848 Gustavus Crusell, a Russian, published a communication on cautery by galvanism. He seems to have been the first to suspect what might be done by this method. He recommends it in anchyloblepharon and symblepharon, also for the extirpation of tumors. He made use of wire and of platinum foil for cutting, and of the latter for cauterizing a surface. In 1846, two years previously, a paper of his on "Galvanic Cautery" had been read before the academy of St. Petersburgh. In 1847 he operated upon a vascular tumor covering a great part of the forehead and region of the eye. He also opened the meatus urinarius, which in consequence of a chancre had become nearly closed. The first operation was performed by moving "to and fro" a platinum wire connected with a battery, and heated in the middle to a white heat, sawing the tumor off, as it were.

Sedillot, in his treatise on operative surgery (1853) refers to the publication in 1849 of the perfect cure of an erectile tumor by the use of the galvanic cautery, and says that MM. Nélaton and Maisonneuve have also employed it.

In 1851 John Marshall of London published an article "On the Employment of the Heat of Electricity in Practical Surgery," in the Medico-Chirurgical Transactions. He refers there to a case which is reported in the Lancet of May of the same year. A young man, 20 years of age, of a strumous habit of body, had a fistulous opening in the right cheek from a succession of abscesses. He had been under a variety of treatment for several months, without success. A fine platinum wire was passed through the fistula, so that its ends could be connected with a battery. The electric current was kept up for nine seconds. But lit-

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tle pain was felt. Sloughs appeared on both orifices of the fistula; that on the inner surface came away on the fifth day, that on the outer one on the sixth. The inner opening was closed on the eighth day, the outer on the eleventh. A small sinus was discovered some days afterwards on the inner surface of the cheek, and was cured within a fortnight by a repetition of the operation. The author mentions having used it with equal success in rectal fistula, and in external and internal hemorrhoids. He anticipates that in some cases this way of operating will be found advantageous as compared with the knife, scissors or ligature.

The Lancet of the same year contains the experiments of MM. Harding and Waite, both dentists. Harding owes his attempts to the perusal of Mr. Marshall's case.

In the Gazette des Hopitaux, 1852, Mr. Nélaton mentions having used this cautery in different cases, with perfect success.

Mr. A. Amussat (in the Comptes rendues de l' Académie des Sciences for July 1853) has used this method of cauterization in ulcers of the neck of the uterus, the extirpation of tumors, &c.

Ellis (Lancet, 1853) cauterizes likewise the neck of the uterus; he recommends it in prolapsus of the uterus, and of the vagina.

By far the most important work on galvanic cautery that has yet appeared, is the one of Prof. Middeldorpf, of Breslau in Prussia.* If not the first to have used it, it is to him we are indebted for our present advance in it. He has certainly made this way of operating easy and practical. It was the perusal of Harding's method of destroying the dental pulp, that first caused him to devote his attention to the subject. The battery he prefers is a large Grove's battery, composed of four cells. The zinc cylinders are six inches long and four broad, each having about seventy-eight square inches of surface; the interior surface being alone reckoned. The positive element is composed of three pieces of platinum foil, each of which being nearly four inches long and three in breadth, the surface presented by it is over sixty square inches; so that the battery can be said to present two hundred and fifty square inches of surface of platinum, and two hundred and ten of surface of zinc. By a very ingenious arrangement of the rods connecting the several elements, he can have a strong or weak current at pleasure.

* Die Galvanocaustik, ein Beitrag zur Operativen Medicin. Von Dr. A. T. Middeldorpf. Breslau. 1854.

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[Sturgeon's pile and the battery of Daniel are also good. Wollaston's battery, which can be constructed at small expense, will answer in many cases.—Axenfeld's Abridgment.—*Editor.*]

The instruments Professor Middeldorpf uses are simple in construction and very easy to handle.

The first (the knife) is composed of two metallic tubes or rods running parallel through a wooden handle, and connected at one extremity by a platinum wire, of different shape for different operations, while the other extremities connect with the poles of a battery. One of the rods being divided obliquely within the handle, the circuit is broken. (Fig. 1.) By pressure on a button connected with one end

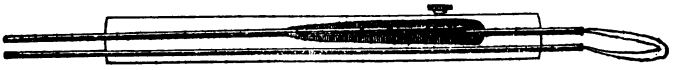

FIG. I.
Middeldorpf's galvanic cautery for the division of tissues.

of the divided rod, the ends are brought in contact and the circuit closed. The wire becomes immediately heated to a red or white heat, according to its size, and divides the tissues as easily as a knife. The wire is always of platinum, this metal requiring a very strong heat to melt it.

By means of this instrument, fissures and cavities can be burnt, abscesses opened, tumors removed, fistulas laid open, &c. The only difficulty consists in heating the wire to the appropriate temperature. If too hot, it acts too easily, and does not prevent hæmorrhage; if not hot enough, it adheres to the tissues and causes, likewise, hæmorrhage on being detached. Pain during the operation is generally pretty severe; after it, comparatively very slight.

Heider's instrument was on this plan; also Harding and Waite's. The latter used a lever instead of a button. Professor Middeldorpf now prefers a wedge, capable of being pushed forward to close the circuit, and backwards to break it; in this manner the continual pressure on the button is dispensed with.

The principle of the second instrument is the same as that of one just described. The difference consists in having a piece of platinum foil bent like an arch instead of the wire. This is useful for burning large surfaces, as in the vagina, rectum or pharynx (where the heated metal serves as a lamp to work by.)

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Ellis (Lancet 1851) invented a very practical instrument for cauterizing the neck of the uterus. At the end of the rods is a small porcelain crucible, heated by the wire that winds around it.—Fig. 2.

FIGURE II.

The porcelain olivary cautery.

[The porcelain ball of the olivary cautery should be extremely thin, that it may be readily heated. It should be grooved to prevent the wire from slipping, with an orifice at its extremity through which the wire passes back into the instrument.—Axenfeld's Abridgment.—*Editor.*]

The instrument for cauterizing the lachrymal sac is bent at a right angle, and the wire is movable.—Fig. 3.

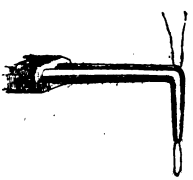

FIGURE III.

Cautery for the lachrymal sac.

[This instrument consists of a small handle, traversed by two demi-cylindrical copper wires, which, after they leave the handle, present, successively, a vertical portion, where the electrodes are adjusted, and an horizontal portion. The latter form a sort of hook, and are perforated for the admission of a double loop of platinum wire. To avoid burning the edges of the wound in the sac, the apparatus is completed by the addition of a little ivory speculum.—Axenfeld's Abridgment.—*Editor.*]

For operating upon strictures the instrument is either straight or has the curve of the male catheter, (Fig. 4,) and

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can be enclosed in an elastic bougie to prevent any danger of burning the mucous membrane of the urethra.

FIGURE IV.

Middeldorpf's galvanic bougie, or cautery for strictures of the urethra.

[It is analogous in some respects to the first instrument described. Instead, however, of the copper wires, two demi-cylindrical copper rods run through an ebony or ivory handle, provided, as before, with a contrivance for interrupting the current. On leaving the handle, the rods run along parallel to each other, forming a cylinder, of which the component demi-cylinders are isolated by an intervening slab of ivory. The cylinder is either straight, or has the curve of the male catheter. The demi-cylinders of copper are connected by a little cone of platinum, which fits on to their anterior extremities. The cylinder, thus arranged, is inserted into a gum elastic catheter, by which the mucous membrane adjoining the stricture will be protected from the heat of the conducting rods.—Axenfield's Abridgment.—*Editor.*]

The principal instrument, however, that Professor Middeldorpf makes use of, is the platinum loop.—Fig. 5. Two

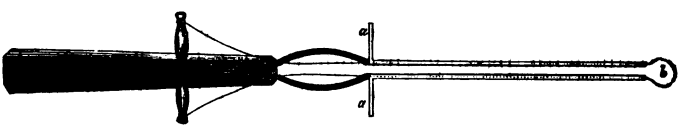

FIGURE V.

Galvanic porte-ligature, cutting loop, *ligatura candens*, *a a*, the extremities of the tubes connecting with the poles of a battery, *b*, the loop of platinum wire, which runs back through the tubes to connect with axle on the handle, by which it may be tightened.

tubes, running parallel one to the other, are supported by a wooden handle; their ends (*a a*) connecting with the battery, are bent. They enclose a wire forming a loop (*b*) at the other extremities, and issuing from the tubes through an opening at the point of curvature. The loop is capable of being tightened to any desirable extent by drawing the free ends of the wire. For greater solidity the tubes are separated by a piece of ivory or other nonconducting substance,

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to which they are attached; and the wire can be wound round an axle, made for that purpose, by which way an uniform tightening of the loop can be obtained.*

[In those cases in which the galvanic cautery was applied to the division of tissues by means of a simple wire, to which a sawing motion was impressed, it was inferior, in some respects, to ordinary incisions, for plenty of room was requisite, otherwise the necessary manipulation could not be accomplished. With the aid of my instrument, however, clean sections can be made in narrow canals, or at the bottom of deep cavities, inaccessible to the knife, and to be reached only by the ligature.

In using the *cutting loop*, it is often desirable to carry the wire through long tubes. It is not necessary that these tubes should isolate the wire; for, in this case, the whole length of the wire would be heated, which would require batteries of great force, and involve the risk of melting the wire. Consequently, I pass the wire through two metallic tubes. separated by ivory or other nonconducting substance. The current passes readily along these tubes, and there is no elevation of temperature except in the projecting platinum loop, which alone offers resistance to the passage of the current. The platinum wire is movable, and can be lengthened or shortened. The loop is never very large; every portion of it is closely applied to the tissue to be divided, whereby fusion of the metal is prevented. A comparatively small amount of electricity develops an intense heat in this short loop.

The porte-ligature which I employed at first, consisted of an ebony handle, connected with two curved pieces of brass, to which the conducting metallic tubes were attached. The platinum wire, passing through these tubes, formed a loop in front, and, escaping through an opening in the tubes at their junction with the brass pieces, it ran backwards to an axle. By turning this axle, the loop was enlarged or diminished. This instrument is very simple, and, in a practiced hand, answers an excellent purpose. It is requisite, however, that the traction by the axle should be steady and uniform; and to insure this, and prevent the accidents resulting from too rapid tightening of the ligature, I have devised a more complicated instrument, in which the ligature is tightened by a rack and pinion.

Galvanic setons are platinum wires of different sizes, which

* We have taken the liberty of adding to the description of Dr. Coolidge the more comprehensive remarks of Professor Middeldorpf himself, which we obtain from the abridgment in the Archives Génerales.—*Ed. Va. Med. Journal.*

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are conducted by means of straight or curved needles, through canals or tissues in which it is desired to develop inflammation.—Axenfeld's Abridgment.—*Editor.*]

Middeldorpf performed his first operation the 30th of March 1853; the removal of a fibrous polypus in the posterior nates. No hæmorrhage occurred. The following May he removed a laryngeal polypus, and has since then performed over sixty different operations, among which can be mentioned the cauterization of fistulæ, the destruction of a large erectile tumor, the perforation of a callous stricture of the urethra, and the removal of uterine and nasal polypi. A summary of the principal cases will not be without interest.

1. *Neuralgia.*—An old woman, 75 years of age, was suffering from neuralgia, having its starting point in one of five or six hæmorrhoids situated around the anus; the pain, especially during defecation, was intense, and had caused the patient great suffering for months. In the operation the hæmorrhoid was first compressed by the heated wire, which was afterwards carried around its base. Some drops of blood appeared; the pain was slight and of short duration. Opium was given to prevent a stool. The next day the neuralgic pain had disappeared, and the wound smarted but little. After five days the opium was discontinued. The first stool, produced by an injection, was very painful. After sixteen days the patient was discharged well. The hæmorrhoid had disappeared, and the neuralgia never returned.

2. *Fistula.*—A workman, 44 years old, was admitted into the hospital suffering from a fistula near the right trochanter. Its direction was from behind forward; its length about three inches. The patient had been previously treated without success, by pressure, by injection of nitrate of silver, of tincture of iodine, by artificial formation of a second opening, and the introduction of a seton. The operation consisted in introducing a double platinum wire, the extremity of which was pushed through an artificial opening. The galvanic current was then passed through the wire. The operation lasted a minute; the pain was slight. After the withdrawal of the wire, a cord could be felt under the skin about the size of the little finger. The next day the openings being closed, the crusts were removed and a teaspoonful of matter escaped. Injection with lukewarm water; no dressing. On the fourth day the openings were pretty clear and surrounded with healthy granulations. Secretion and swelling moderate. The artificial opening healed on the twenty-sixth day; the posterior one on the fourteenth. Soon after, the patient was discharged well.

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3. *Fistula in ano.*—A man, 48 years of age, was suffering from an abscess near the sphincter ani, giving rise to a fistula an inch and a half in length, and running parallel to the rectum. No internal opening could be found, and after an unsuccessful treatment by enlargement and the use of tents, the galvanic cautery was tried. An artificial opening was made into the rectum; a platinum wire passed through the fistula, and coming out of the anus formed a loop, which by its appropriate instrument having been put in connection with a battery, the whole part was divided, as in the common operation. No hæmorrhage; slight pain. After two days, healthy granulations appeared, and at the end of a month the patient was discharged well.

4. *Erectile tumor.*—A boy, three months and eight days old, had an erectile tumor situated on the left side of the face. It reached from the zygomatic arch to the mastoid process, about two and a half inches in breadth, and from the meatus auditorius to three-quarters of an inch below the inferior maxilla. The tumor is raised about one inch and a half above the surface of the face. It is yielding, fluctuating, warm, without pain, and does not pulsate; swells out during inspiration and during crying; it can be compressed and emptied like a sponge; then two small arteries are felt beating. The child is healthy and strong.

Three unsuccessful attempts were made to obliterate it by galvano-puncture. Two needles were passed into the tumor and put into communication with the positive pole of a battery. The moist skin of the tumor was touched with the negative pole. No coagulation took place, though the operation lasted fifteen minutes. The needles, on being withdrawn, were still bright. The small openings they had made were hardly cauterized. No reaction followed. Two weeks afterwards the operation was repeated. The two needles were placed at right angles, and alternately touched with the positive pole of the battery for half or three-quarters of a minute. The negative pole was in contact with the skin. After fifteen minutes the needles were withdrawn; this caused some drops of blood to appear. The needles seemed, as it were, baked to the parts that surrounded them, and some force was necessary to draw them out. The tumor was a little contracted, and had become reddish. Two hardened ridges could be felt in it. No reaction occurred. Some days afterwards the tumor had again become soft and yielding. The diameters were about the same as when first measured, but it had become more prominent. Galvano-puncture was used the third time. Five needles were

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passed into the tumor. The operation lasted twenty minutes. After the withdrawal of the needles, the whole surface was covered with collodion. The tumor, which had become contracted during the operation, became now still smaller; its red color disappeared. For some days it remained in this state, but for a short time only. A fortnight after the operation, it was found to be larger than it had ever been before. Injections were considered unsafe in this case, and as a last resort the ligature of the carotid was proposed. This was postponed, and after six months it was resolved to try the effect of the galvanic cautery; though with little hope of succeeding. The platinum wires were passed into the tumors, crossing each other at right angles. They were allowed to remain incandescent for ten or fifteen seconds. They were then withdrawn, not without difficulty, and on account of their adherence to the cauterized tissues some hæmorrhage occurred.

The tumor gradually collapsed, pulsation ceased in it, it no longer swelled during the crying of the child; and at the end of four weeks it was no larger than a walnut. A portion beneath the jaw, which had escaped cauterization, enlarged, but after an operation similar to the first, it diminished in a remarkable manner, and never again regained its former size.

Entropion, trichiasis and districhiasis have also been successfully treated by the same remedy.

5. *Callous stricture of the urethra.*—A patient, 50 years of age, in consequence of frequent attacks of gonorrhœa, was suffering from a callous stricture of the urethra, of about one-quarter of an inch in length, and which would only admit a bougie of one-fifteenth of an inch in diameter. The urine dribbled away, drop by drop. The patient was put under the influence of chloroform. An instrument, having the curve of the male catheter, was passed up the urethra and pressed against the stricture. The connection between the instrument and the battery being formed, in ten or fifteen seconds the stricture was perforated. Four weeks after the operation the patient was discharged well. Nine months afterwards, a zinc catheter, about four lines in diameter, could be passed with perfect ease.

6. *Polypus of the larynx.*—A minister, 42 years old, was suffering from a polypus of the larynx, situated above the right vocal chord. The respiration was loud. It was with great difficulty the patient could speak in an audible tone. Swallowing solid food was extremely difficult. The cervical glands were swollen. On opening the mouth the free end

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of the tumor could just be seen behind the epiglottis. The operation consisted in seizing the polypus with a pair of forceps and throwing the loop of wire over it. By tightening the loop it slipped towards the pedicle. The galvanic cautery was passed through the wire, and the polypus drawn out by the forceps. After four days the patient left his bed, and on the fifth his room. Five weeks after the operation he had resumed his duties. One year and a half afterwards, an examination proved that up to that time the cure had been permanent.

7. *Polypus uteri.*—A woman, suffering from polypus uteri, which had been mistaken by some nurses for a prolapsus and treated accordingly, was seized during the night with diarrhœa. The next morning a pediculated tumor, of the size of a child's head, was detected just within the orifice of the vagina. Five days afterwards, fever having set in, the state of the patient became such that the worse prognosis was formed. When the operation for removing the polypus was resorted to, she had been for several days lying in bed, pale and emaciated; she was exhausted and perspiring profusely. Between the thighs could be seen a round and elastic tumor, of a pale red color; three-quarters of which were outside of the vulva and covered with mucus. The loop of wire was passed over the tumor; on tightening it, it slipped upwards towards the fundus of the uterus; the connection with the battery being formed, the tumor was removed. No hæmorrhage occurred. The cut pedicle was about the size of a quarter of a dollar. The polypus weighed one pound and nine ounces. The patient left her bed four weeks after the operation. This was owing to the state of exhaustion she was in; for in another case of removal of uterine polypus, the patient was discharged well four days after the operation.

8. The last operation we wish to mention is the removal of a fibrous tumor, growing from the posterior part of the pharnyx at the base of the skull. The patient, a young man of 20, had already undergone an operation for its removal. This operation consisted in opening the nose on the right side, and cutting the tumor off with scissors. Great hæmorrhage had occurred, amounting to the loss of over three pounds of blood. Six weeks afterwards the polypus returned. It was situated in the right cavity of the nose, and forced the septum very much to the left. It could also be felt by the mouth behind the palate. The right eye was slightly pushed forward. A sound could be passed to the left and to the right of the tumor into the pharnyx.

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Four different and unsuccessful attempts were made to surround the pedicle with the loop. During the second attempt, a part of the tumor was cut off. Notwithstanding its great vascularity, no hæmorrhage occurred; no pain was felt. At the fifth attempt the loop was passed around the tumor, and it was successfully removed. It was found to weigh one ounce and one drachm. No bleeding followed; the pain was very slight, the patient complaining only of a sense of warmth in the neck and nose. The remaining root of the polypus was extirpated with scissors, not without great hæmorrhage. The patient recovered slowly.

In another case a similar tumor weighing three ounces, was removed by means of the platinum loop without the slightest hæmorrhage. The patient, a boy 11 years old, was discharged after fifteen days.

For the removal of internal and external hæmorrhoids, Middeldorpf's loop has proved very successful.

It is difficult to say how large an artery may be cut in this way without producing hæmorrhage. We think, after the experiments we have seen and made, that any artery the size of a common quill can be cut with perfect safety; and perhaps much larger. The principal difficulty is in regulating the heat of the wire and the quickness of the hand.

The wire can be too hot or too cool; in the first place it divides the tissues without singing them, and does not prevent hæmorrhage; in the second place, it adheres to the tissues which are lacerated when it is removed, equally producing bleeding. If the wire is moved too rapidly, it does not singe; if too slowly, it carbonizes the parts instead of contracting them. In both cases, hæmorrhage occurs.

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CHRONICLE OF MEDICAL SCIENCE.

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *Connection of Dentition in Children with Nursing and Weaning.* A Clinical Lecture at the Hotel-Dieu. By M. Trousseau. Translated for the Charleston Medical Journal and Review.

The most elementary questions in medicine are often the least known; and although it might seem unnecessary to pay much attention to matters of apparently so little interest, and which swarm about the practitioner, yet you should remember that Stoll wrote a chapter, entitled "*De quibusdam magni momenti minutiis,*" and accustom yourselves to neglect nothing.

The infant has twenty teeth—the youth twenty-eight—the man thirty-two. The infant's number is not complete until the thirtieth or thirty-sixth month; but they are only temporary teeth, for at the age of seven years he begins to lose them, and to acquire others more durable; at thirteen or fourteen years, this exchange is physiologically accomplished. Except that great king, who was an exception in everything, and who was born, it is said, with two teeth, the child comes into the world with two unarmed maxillaries, and it is not generally before the eighth month that the first milk teeth make their appearance; but as the laws of nature are somewhat capricious, it not unfrequently happens that one child may have teeth at four months, while another at a year old is still without them; thus no precise limits can be fixed. Generally the two middle inferior incisors appear first, and I always anticipate a troublesome dentition when the superior teeth appear first. These first two teeth always appear together, with an interval between them varying from one to eight days; but *together*, mark it well, and this is the case with these two only. Six or eight weeks after, the two middle superior incisors make their appearance, but not together, and with an interval of eight to thirty days. The process of dentition, then, is rapid with the first two teeth, and slower with the others. Then two

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more are evolved, the two lateral superior incisors, in a month or two after the superior middle incisors; thus in about a year, the child has six teeth, commencing with two inferior, and continuing with four superior.

The teeth of infants come out in groups, *dentes in infantibus catervatim erumpunt.* The first group consists of two middle inferior incisors at about eight months; second group, two middle superior incisors about ten months; third group, two lateral superior incisors at about a year; fourth group, two lateral inferior incisors and the first four molars (six teeth in this group) at fourteen to sixteen months; fifth group, four cuspids at eighteen months to two years; sixth group, four second and last molars at thirty or thirty-six months.

The cuspids appear after the child has completed twelve teeth, and is eighteen months or two years old; their evolution occupies about two or three months. There is then an interval of six to ten months; and at the age of three years, when it has brought out its last group, the process of dentition is over.

I have not spoken of groups undesignedly; for you will see that, in connection with weaning, it requires attention. An important fact is that, after the appearance of each group, the child has a short period of repose; and this is the propitious occasion for weaning. Generally no regard is paid to this; infants are weaned indiscriminately, when they have two, seven, nine, eleven, or fourteen teeth. But you must observe closely; otherwise you will lose your little patients by that terrible affection of the bowels, cholera infantum.

You will, doubtless, be often consulted about the time for weaning; never decide until you have closely examined the circumstances of dentition, and do not authorize the mother to wean her child until it has six, twelve, or sixteen teeth. In the best practice, weaning should never begin in the interval after the evolution of the first group; the subject is too young, and is usually only eight months old. And it is only with very great caution, that you should begin after the third group—if strongly desired by the parents you may consent; for there is a space of a month or six weeks of repose before the coming out of the fourth group; but be careful to remember that the child has only six teeth, that it is only a year old, and that foreign nourishment will not always answer your expectation.

The most favorable time for weaning is, undoubtedly, the interval which separates the fourth from the fifth group; at that epoch, the child is supplied with twelve teeth, eight

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incisors and four molars, and has before it a sufficiently long period for repose, about two months, during which there is nothing to apprehend from the bowels; thus when the cuspids begin to appear, (and the evolution of this group is the most dangerous,) it is habituated to its new diet, and prepared for the crisis which awaits it.

Wait, then, for the fourth group before weaning. If the health of the mother, or nurse, or family interests, oblige you to authorize premature weaning, do so when there are six teeth; but if not forced to yield to some such consideration, defer until there are twelve.

Do not suppose that matters will, in all cases, progress thus regularly. You will probably see some children cut their molars before their incisors, or the superior incisors before the inferior; for although dentition proceeds ordinarily as I have indicated, yet there are often irregularities which are very embarrassing to the physician, whose chief desire is to ascertain some interval of repose. Do the best you can; examine the state of the gums and wean as soon as possible after the evolution of a tooth, as there will then probably be a brief respite.

Among the accidents to which the period of dentition is liable, the gravest and most tenacious are those of the alimentary canal. Several days before hand, the child is restless, it sleeps little, utters violent cries, sucks its fingers, presses the nipple of the nurse, refuses to take any supplementary nourishment, and sometimes even to nurse; the gums are red, and there exists a very perceptible elevation at the point at which the teeth are to appear; it coughs, the voice changes, the mucous membrane of the mouth is irritated; but as soon as the child has two teeth, the surrounding gingival tissue will become very red and tumefied.

If you administer mercury to an individual who has not a tooth in his head, and who wears false teeth, neither salivation nor mercurial stomatitis will ensue; but should this person have a single tooth remaining, mercurial manifestation will take place around it; the surrounding tissue of that tooth will be inflamed, while other portions will not be affected by the inflammation. Now the case is the same for the first teeth of the infant; their issue has no effect upon the gums; but at the evolution of the second group, the gums redden and swell.

With nearly all children, the process of dentition is accompanied with diarrhœa; moderate sometimes, but often intense, with evacuations of a greenish color, and resembling a hash of green herbs, or clots of curdled milk, containing

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glairy and even sanguineous matter. In certain cases, there is much tenesmus, with prolapsus of the rectum. These accidents, which precede by several days the issue of the tooth, often continue until the whole group is cut. Should the diarrhœa not cease, you will resort to all the necessary means to restrain and ameliorate it. During this diarrhœa, weaning should not be permitted, unless the milk of the nurse should appear to you to contribute to the internal flux.

In the summer season, the disorders attending dentition are generally of the intestines, rarely of the respiratory passages. Intestinal derangements, fever, peripneumonic catarrh, and other pathologic manifestations concerning the lungs, are of frequent occurrence in the winter.

I must warn you against a certain vulgar prejudice, and enjoin upon you to combat it whenever presented. You will hear it said and repeated that *diarrhœa is good for children:* it is not so, and it will often cause the death of your little patients. Diarrhœa brings on chronic enteritis, and this debilitates and carries off children. By combatting, therefore, this intestinal flux, you will guard the better against affections in other parts.

It is also considered improper to remove the scurf which frequently covers children's heads; but this ridiculous prejudice has ceased in England and in the United States; let us disregard it here.

If, during dentition, the evacuations should become somewhat more easy, without degenerating into diarrhœa, you need not fear it; but should it continue several days, it must be checked.

The opinion is current, that constipated children are often attacked by convulsions, while those having diarrhœa are never thus affected. This is not the case; diarrhœa is a frequent source of convulsions, and a healthy state of the bowels generally prevents them.

I call your attention particularly to alimentation, for it is a matter of the utmost importance. Indeed, if you neglect it, diarrhœa will ensue, and successively enteritis, serious indigestion and convulsions. Nothing is more common than indigestion, induced by enteritis, and leading to convulsions, and nothing is more calculated to alarm parents; they generally lose all self-command; and while the servants or the neighbors hurry off for a physician, the mother, under the advice of some over-officious old woman, pours boiling water over the feet and hands of her child, and scalds it often to death. And this reminds me of what once occurred to an

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eminent colleague, Professor Marjolin, during the course of a typhoid fever, which had thrown him into a profound stupor. They applied to his legs towels dipped in water at 190°, causing large eschars, which were not healed for several months.

With regard to convulsions, the less you do the better; indeed, the paroxysm is generally over before you arrive; and although the symptoms should reappear two or three times, yet by the next day everything is usually over. If there has been any indigestion, administer a laxative, for it is best to get rid of undigested food; let the child nurse very little; give it mucilaginous drinks, have it bathed, if necessary, and you will soon cause the disappearance of all alarming symptoms. In general, everything succeeds, even the infinitesimals of the infinitely ridiculous homœopathy.—*Gazette des Hospitaux.*

2. *On the Effects of Lead on the Heart.* By JOHN W. CORSON, M. D. Physician to the New York Dispensary.

A careful analysis of ten cases of lead poisoning coming under his observation at the New York dispensary, has led Dr. Corson to believe that, in addition to the symptoms and tests usually relied on in the diagnosis of this insidious malady, we may add another, which, according to our author, is as invariable, as far as his observations at present extend, as the leaden streak of the gums, which we may rely on as the pathognomonic symptom of this morbid condition. Dr. Corson cites his cases in full, and finds, as an invariable symptom, a *greatly diminished cardiac impulse*, accompanied by mitral regurgitation and faintness on the slightest exertion. In some of the cases, the impulse of the heart was greatly diminished. In others, the most alarming syncope would occur, accompanied with palpitation. In all the cases, the peculiar *violet streak of the gums* existed. Dr. Corson concludes that *lead*, like digitalis, oil of tobacco, upas antior, the woorari, and some other poisons, tends to paralyse the central organ of circulation, and like them, will ultimately (although much more slowly) produce what Bichat termed *death by the heart.*

3. *On the use of Acetate of Lead in Hypertrophy of the Heart.* By J. L. BRACHET, M. D. Read to the Academie de Médecine.

Our readers have observed, in our present issue, the interesting remarks of Dr. Corson of New York on the action

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of lead on the heart and its controlling influence on the cardiac impulse. As corroborative of the opinions of Dr. Corson, we prepare a brief abstract from the New Orleans News and Hospital Gazette, of a paper read by Dr. Brachet to the French Academie of Médecine upon the therapeutic uses of lead in hypertrophy of the heart, with *increased cardiac impulse.*

Dr. Brachet's cases are very forcible, and would lead us to conclude that the use of the acetate of lead, with or without digitalis in combination, certainly must, as Dr. Corson insists, exercise a very powerful influence over the impulse of the heart. In Brachet's first case, the disease had existed for three years. The heart beat with great force. All his symptoms indicated hypertrophy of the right ventricle. Bleeding, digitalis, hydrocyanic acid—all had failed. The use of sugar of lead and the extract of digitalis for some months, the treatment being occasionally discontinued, resulted in a cure. A second case, where there was greatly exaggerated cardiac impulse, and which had resisted treatment for three months, was controlled by the lead in ten days.

Dr. Brachet concludes by declaring that he believes "in the special electric action of lead upon the heart, as he does in the action of mercury on the salivary glands, cantharides on the bladder, or diuretics on the kidneys;" and we must agree that the observations of Dr. Corson and the experiments of Dr. Brachet alike prove the probable correctness of this statement.

4. *On Scarlatina.* By Dr. Brugerier.

Considerable attention has been paid of late to the remedial powers of the Brazilian root *cahinca*, (chiococca racemosa,) which is mentioned in our dispensatories as having disappointed the expectations raised by Dr. Francois' statements in regard to its efficacy in dropsies. Continental physicians publish observations showing that cahinca is a good diuretic, and Dr. Brugerier claims (Revue Therap. du Midi) that it surpasses all other remedies in scarlatinal dropsy. His enthusiasm induces him to overrate its merits, perhaps; yet he adduces six cases in which signal success attended its use. He directs that two drachms of the root should be macerated in a half pint of water and boiled. The decoction is given in three doses, with intervals of two hours. "It produces an extremely abundant flow of urine."

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SURGICAL PATHOLOGY AND OPERATIONS.

1. *Entrance of Air into Veins during operations.* From the Reports of the London Hospitals.

An interesting instance of the passage of air into the veins during an operation for torticollis is reported in a late number of the Medical Times and Gazette, as happening in the London hospital, under the care of Mr. Ward. The patient, just as the knife divided the sterno cleido muscle, fell back insensible, and at the same time a loud hissing or sharply blowing sound was heard. Mr. Ward at once placed his finger on the wound, and artificial respiration was commenced. In a few minutes the child recovered.

A second case, analogous to the above, occurred under the care of Mr. Hutchison of the Metropolitan free hospital. Whilst extirpating a cancerous gland in the axilla, a whizzing, bubbling, very peculiar noise was heard, and the pulse of the patient immediately sank. The assistant placed his finger on the wound, when the noise ceased, and the patient rallied. After waiting a few minutes, the arm was raised and the finger was removed, and again the peculiar whizzing returned. The finger was replaced, and after further delay, the operation was concluded, all the vessels being tied. The reporter concludes the history of these cases with the following remarks:

The entrance of air into veins is a matter of so much interest to the practical surgeon, that we may, perhaps, profitably, in connection with the cases just cited, bring before our readers the following notes concerning it:

1. *Symptoms.*—A whizzing, gurgling sort of noise should always be regarded as sufficient to excite the utmost alarm, and on hearing it, the operator should not allow a moment to escape.

2. *Treatment.*—The finger should be instantly placed upon the wound. The patient should be laid in the recumbent posture, and, if syncope has occurred, artificial respiration should be commenced. If apparent death has ensued, endeavors at resuscitation should, for reasons about to be given, be long persevered in.

3. *The entrance even of considerable quantities of air into a vein is not nearly so fatal an accident as generally supposed.*—The writer once introduced a small injecting tube into the jugular vein of a horse, and blew as forcibly as he could for some minutes. The animal appeared uneasy, but nothing more. A large tin tube, with bore as great as the thickness of a little finger, was next procured, and half an hour or more having elapsed, and the horse seeming quite well, the wound was reopened, and a second attempt made. A very large quantity of air having been

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blown in, the horse at length fell. His death was slow, and seemingly very painful, occupying at least twenty minutes. After death the right side of the heart was found literally distended into a bladder of air. The result of this experiment quite accords with those obtained by many other observers. Dr. Blundell* injected 5 drachms of air into the femoral vein of a small dog; no symptom of immediate danger ensued, and in three days the animal was as well as ever. So common, indeed, has it been for recovery to follow the symptoms of entrance of air into a vein. that M. Ph. Boyer actually goes the length of denying that any proved instance of its causing death in the human subject is on record. M. Vidal also has insinuated that the accident is one invented by surgeons to account for the sudden death of their patients. Whilst it cannot be needful to demonstrate the absurdity of these opinions, the circumstance of their having been held by such surgeons is instructive as pointing to the rarity of the occurrence.

4. *Air may be easily absorbed from the interior of the large vessels.*—This follows necessarily upon what has just been advanced. If death does not occur quickly the patient will probably recover. No endeavors should therefore be made to remove the air at the time, nor should any fear of its "decomposing the blood" be entertained. All the surgeon has to do, is by artificial respiration, galvanism, ect. to keep his patient alive for a certain time.

5. *Proportion of deaths in recorded cases.*—In an able review of Dr. Wattmann's book on this subject, published in the Medical Review for July 1847, a tabular report of all recorded cases known to the writer is given. The list comprises 54 cases in which the accident occurred to the human subject, and of these 29 ended in death and 25 in recovery. While in several of these the evidence is very incomplete, in a majority there can remain no reasonable doubt as to the reality of the occurrence.

6. *Length of time elapsing before death.*—In most of the fatal cases given in the table referred to, death occurred, if not instantly, within a few minutes. In one, however, it followed fifteen minutes after the conclusion of the operation; in a second, "some hours" afterwards; in a third, suddenly, three hours afterwards (no autopsy;) in a fourth, on the seventh day, from bronchitis; and, in a fifth, on the twenty-eighth day, the immediate cause being pneumonia. In the last no autopsy was made, and, as the patient was a young infant, it is very doubtful whether the inflammation of the lungs had any connection with the accident under consideration.

2. *On the Comparative Value of Laryngotomy and Tracheotomy in the Treatment of Croup.* By J. P. Batchelder, M. D.

Although the weight of authority is at present much in favor of the operation of tracheotomy in croup, in comparison with the kindred operation of laryngotomy, and in spite of Trousseau's great name and immense experience enlisted strongly as the advocate of the former operation, we yet agree with Dr. Batchelder, in his very interesting paper, lately published in the New York Medical Times, in the preference he there expresses for the opening into the larynx

* Medico-Chirurgical Transactions, Vol. IX. p. 65.

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rather than the trachea; and we give a very brief abstract of his remarks on this subject.

The objections to tracheotomy are: 1st. The danger of wounding the thyroidal veins, and the danger of hæmorrhage or of blood getting into the trachea. 2nd. The irregular distribution of the arteries in that vicinity. 3rd. The thyroid gland in children is often in the way. 4th. The depth and small size of the trachea. 5th. The deficiency of the fibro-cartilaginous rings at the posterior part of the trachea, and hence the tendency of the lips of the wound to come together, which renders necessary the introduction of a tube.

The objections to laryngotomy are: 1st. The cutting into the inflamed part. Here Dr. Batchelder asks why we do not object to scarifying and excision of the tonsils or the gashing of carbuncles? How often do deep incisions into inflamed parts *cure the inflammation*, as in erysipelas? 2nd. The intolerance of the tube is urged against the opening into the larynx. Our author here quotes from Thompson and Ericksen of London, where the tube in the larynx had been patiently borne for six or seven years.

3. *On the Treatment of Syphilis.* By Dr. Hanselmann. (Wiener Medicin. Zeitung, No. 42.)

In this paper, Dr. Hanselmann describes a somewhat novel treatment, which he states he has found very successful in military practice. 1. He prefers to all other means the employment of lemons and corrosive sublimate in the treatment of indurated chancre, syphilitic eruptions, condylomata, mucous tubercles, periostitis, &c. Two lemons are eaten, peel and all, daily, for a fortnight, and then one-eighth of a grain of sublimate is given daily for a week. This three weeks' course, he states, is in general sufficient, and a repetition of it is seldom required. In the summer, the patient takes a bath daily. 2. The local treatment of chancre consists in employing, three times a day, a penis bath, formed of two drachms of alum to one pound of water, and continued until a reddish circle is formed around the sore, and a yellow membrane is detached from its surface. The ulcer is, for the most part, left of a pure red, its bottom being either quite even or irregular. In the first case, an indurated cicatrix results from the continuance of the alum bath, and it is better to substitute one of diluted acetic or tartaric acid, which induces a far better cicatrix, though its formation requires more time. In simple chancres, the bottom of

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which is not yellow, but mingled red and yellow, the acid bath should be used from the beginning. No yellow membrane is here cast off, but the sore gradually cleans, and an even cicatrix is formed. The healing is more tedious, but neither induration nor bubo occurs during its progress, while they may arise during the use of the alum bath. During the employment of these penis baths, the number of ulcers may become increased, owing to the bursting of small subcutaneous abscesses. The baths are to be continued, and two lemons should be eaten daily for a while. 3. In balanitis, in inflammatory induration, complicated chancre, and in congenital phymosis, the alum baths have been found especially useful. The watery, eczematous ulcers of balanitis heal in a few days, inflammatory indurations disappear in about a week, and for the operation for phymosis, formerly of frequent occurrence, is now substituted the mere scarification of the upper border of the prepuce. Indurations induced by the local application of corrosive sublimate are quickly dissipated by the alum bath, but those which result from the use of blue vitriol, common among soldiers in garrison, are more obstinate. Two compresses, with rectified spirit, are first to be applied, and then lemons and the alum bath resorted to. The same plan has been often successful in the Hunterian chancre, when the induration has not yielded to five or six days' use of alum baths. 4. Broad condylomata are treated by penciling them twice a day with rectified spirit or concentrated acetic acid; and in the few cases of pointed condylomata he has so treated by the vinegar, the author has found a rapid diminution result.

Med. Chir. Review.

TERATOLOGY.

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1. *Three Anencephalous Monsters born of the same Mother.*—By Dr. Bruncel.

In the annals of the Medical society of Bruges, (Belgium), a monthly record of the researches of the medical men of that town, we find the following remarkable history:

A woman of 42 years, residing near Bruges, has been the mother of fourteen children. Of these, the fourth, twelfth,

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and fourteenth were still-born, and presented different degrees of several varieties of monstrosity.

The first is not described, Dr. Bruncel being unable to collect accurate information regarding it.

The second presented a complete absence of the vault of the cranium. The encephalic masses were wanting, or were rather replaced by a reddish flaccid tumor, without a trace of cerebral matter. The spinal marrow ended at the great foramen in a reddish enlargement. There was a *spina bifida*, including three cervical vertebra.

In the third monster, the encephalon could be traced out in some parts. The nervous matter was soft and almost diffluent. It protruded posteriorly as a sanious mass.

The face, in the last two subjects, was disproportionately large. There was double hare-lip in one, single hare-lip in the other; the first had a sixth finger on each hand, and a sixth toe on each foot; the second had a supernumerary finger on each hand.

Mrs. Blank had enjoyed good health in all her pregnancies. In the twelfth, in which she gave birth to the second anencephalic monster, she had a very tedious labor. Podalic version was necessary, which was rendered very difficult by the enormous distension of the abdomen and chest of the fœtus, both of which cavities were the seat of dropsy.

The second monster belongs to the genus *Deroncephali*, of Geoffroy Saint-Hilaire, the third to the genus *Notencephali*. It is well known that there are two principal theories in regard to the general causes of monstrosities; that of *arrest of development*, and that of *morbid atrophy*.

The second monster described by Dr. Bruncel, seems to present some characteristics on which an argument in favor of the second theory might be based. Besides the cephalic anomalies, this monster had ascites and hydrothorax. Now the existence of these serous effusions in the peritonœum and pleura afford strong presumptive evidence, that at an early stage of fœtal existence there was dropsy of the cerebral membranes, resulting from inflammation. Thus we are led to the conclusion, favorable to the theory alluded to, that the absence of the encephalon was due to atrophic causes, and not to an arrest in the transformation of the rudimentary substance of the brain. Perhaps the records of science contain no such striking fact as this in support of the doctrine of *atrophy*.

We remark also another important fact in this case. We refer to the repetition of the same monstrosity in several successive children of the same mother.

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2. *Case of United Twins.* By Dr. Ramsbotham.

On the 4th of April 1855, one of the rarest cases of double formations occurred in the St. Petersburgh Foundling hospital—namely, two girls growing together by the skulls—which still live, and so far appear to be in good health. Of all the cases of this kind hitherto known, (of which there are seven,) the union of the two individuals was never of that kind to bring the face of one child directly opposite to the face of the other. These twins are so united that if the middle line of the face of one child be prolonged from the nose, this would strike upon the ear of the other. Through the mobility of the necks the two children really lie in a straight line, one girl lying on the back, the other on the side, and thus they sleep. The face of one child is quite symmetrical as far as the forehead, and it is first in the formation of the skull that want of symmetry appears. In the face of the other, the right half is much shortened, and the eye of this side opens less than the other. The two children possess a perfectly independent existence from each other as relates to sleeping, waking, want of food, and so forth. The one sleeps quietly, whilst the other takes nourishment or looks about. Common sensibility does not appear to exist, since in cases of this kind the brains and nerves of each individual are preserved distinct. Not so always with the blood vessels. Once one child screaming loud awoke its sister. The face of the screaming child became suffused and reddened deeply, whilst the other was still asleep. Then the face of the other began to redden and swell, and it was only after this that it opened its eyes. The features of the two children, especially of the one whose face is not shortened, are very pleasing. The physicians of the Foundling promise to observe this case more fully, and publish the results.—*Med. Chir. Rev.*

DIETETICS.

1. *On Horse-Flesh as an Article of Food.* By M. Geoffroy St. Hilaire.

Amongst the novelties which have been lately originated by our ingenious French brethren, we may especially notice the Society of *Hippophagi* or horse eaters, composed of some

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of the most eminent French savans, with the well known chemist Geoffroy St. Hilaire at their head.

These gentlemen, after dilating on the general scarcity of wholesome food, have determined to introduce horse-flesh upon their tables until the prejudices which we have against this "wholesome and nutritious food, rich in nitrogen, and not disagreeable to the taste," have been surmounted.

Our repugnance to horse-flesh arises simply from our long having ceased to use it. Anciently, both the horse and the ass were employed as articles of food. The use of horse-flesh was at one time general among the inhabitants of the north and west of Europe. The reason for its disuse is thus given by M. Geoffroy St. Hilaire:

"The worshippers of Odin used the horse in sacrifice. When the animal was sacrificed, the flesh was served up on the tables of the priests and of all classes of the population. The eating of horse-flesh was thus connected with the rites of the Odin religion, and was a great obstacle to the establishment of christianity among the people of the north: for, whenever a Scandinavian, even though converted, ate horse-flesh, his mind reverted to the recollection of his former faith. Hence at an early period the popes prohibited the use of this article of food. In the eighth century, Pope Gregory III wrote to St. Boniface, archbishop of Mayence, to 'abolish the custom by all possible means, and impose a proper penance on all eaters of horse-flesh.' They are unclean, and the act is execrable.' His successor, Pope Zacharias, renewed the interdiction.

"Now that the motive of the prohibition issued by the popes has disappeared for many years, the use of horse-flesh is being gradually resumed; and it is remarkable, that it is first resumed by those who were the latest to abandon it. Denmark leads the way: in that country, horse-flesh is sold publicly under the inspection of the government. For some years, Belgium has followed the example: and recently, the Austrian government has authorized the public sale of this article of food.

"It is to be hoped that France will not be the last country to throw off old prejudices. A wholesome, nutritious, economical article of food, is lost in France by millions: and at the same time there exists millions of individuals insufficiently fed, and physically and morally deteriorated. The use of this article of food would regenerate them, and give to the state a class of robust and intelligent servants. If Ireland had been put in possession of this article of food, that country would perhaps not have offered the spectacle of one entire people torn by famine from their ancestral soil."

2. *Un Diné de Cheval.* By Amedee Latour. (From Union Med.) Translated for the N. O. News and Hosp. Gaz.

"Do you like horse?"
"How do you mean?"
"I mean the flesh of that animal."
"Why, really, I have never eaten any."
"Would you like to taste it?"
"Is it good?"

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"Excellent. Allow yourself to be tempted."

"When and where do you eat horse-flesh?"

"At my house—Saturday evening, at six o'clock, soup, soup meat, and roast are from the horse; what do you say?"

"I accept."

Such was the conversation which, on Tuesday, after the meeting of the Academy of Medicine, took place between M. Renault, the skillful and scientific director of the Imperial Veterinary School at Alfort, and myself.

I confess that from Tuesday to Saturday it was necessary to screw up my courage. But why should we not eat horse? Why should the flesh of this beautiful animal excite any more disgust than that of the ox, the sheep, or of those really unclean animals, such as the hog or the duck? On how many occasions could the meat of the horse enter into the general alimentation, either as a supplement or as an accident! Beneath all this, perhaps, there may be a great question of political economy; let us, therefore, use our influence to solve it. The high price of all butcher's meat is a true public calamity; if it were possible to introduce horse-meat into the general consumption—a nutritive article—would not other meats become more accessible to the masses, &c.?

By such considerations as these did I endeavor to conquer what remained of my repugnance—a repugnance instinctive in man for all aliment which he does not know, and to which he is not accustomed.

I confess that I took the further precaution to make a light breakfast, in order that the edge of hunger might make me less difficult to please. In fact, I arrived at Alfort in a most satisfactory state of appetite.

The moment for the experiment is here. But first, one word concerning the experimenters. They were M. Renault, our Amphyction, who was not making his *début* in this matter, and who professes to hold in high esteem the flesh of the horse. That very morning, at breakfast, he had eaten the liver of this animal, which he declares he had found extremely good. M. Renault commenced, therefore, the experiment with the certainty of success. Professor H. Bouley, the keen and witty opponent of M. Malgaigne, in the discussion on the seton—this experimenter did not appear to me to be as warm in his praises of the horse as his colleague: he acknowledges some slight remains of ancient prejudice; nor was this his first essay, and he did not seem to retain the most agreeable remembrance of his past experience. M. Reynal, chef de clinique, author of a very interesting work on

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the toxocological properties of brine, who shared openly M. Renault's high opinion of horse flesh. M. Robinet, from the Academy of Medicine, the terrible, yet amiable, exterminator of all new and secret remedies; M. Debout, principal editor of the Bulletin de Therapeutique, very well disposed to go through the experiment conscientiously. M. Foubert, chief of the bureau of subsistence in the department of agriculture and commerce, very much interested in the question. M. Prevost, chief of the bureau of agriculture, in the same department. M. Barral, the professor of chemistry, the faithful editor of the works of M. Arago—one of his friends, whose name I have unfortunately forgotten. A lady! yes—too fastidious gentlemen—an amiable and lovely lady, who set us all an example of resolution and courage—and, finally, your humble servant.

One word on the subject of the experiment. It was an old horse, who, after twenty-three years of good and loyal services, had had the misfortune to become paralyzed in his hind quarters. He was, moreover, fat, and be it well understood, free from all suspicion of disease. He had been killed on Wednesday. The experiment was not made under the most favorable circumstances, and M. Renault remarked very truly, that the flesh of a young horse would certainly give much more satisfactory results.

The experiment commences. M. Renault has arranged matters very fairly. Side by side with the subject of experiment stands the point of comparison. Horse-soup, beef soup; horse-soup meat, beef soup meat; same quantity, same quality; compare, judge; nothing could be better.

Horse-soup.—General surprise! This is perfect, it is excellent, it is luscious, it is aromatic, its taste is rich, it is the admirable and classical soup; the tradition of which is fading, unhappily, day by day, from our Parisian household, and which has become a myth even in our best eating-houses.

Beef soup.—Yes, it is good; but in comparison it is inferior, it is less accentuated in taste, less perfumed, less racy.

The jury unanimously declare that horse, even old horse, makes a superior soup; that it is impossible to distinguish it by the taste from the richest beef soup, and no one whose attention had not been called to the subject could perceive the least difference—same color, same clearness.

Horse soup meat.—The flesh is browner than that of beef, dryer and tougher, otherwise, no particular taste; it is the flavor of boiled beef, but not of a first quality; I have eaten better beef, and much that was inferior. To sum up the whole, it is very palatable; and the poor, who buy the worst qualities of beef or of cow, would find a sensible difference

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in favor of boiled horse-meat. But let us remember that we have had to do with an old horse, whereas a young animal would, no doubt, have supplied a superior "bouillie."

Beef soup meat.—This was a magnificent tender loin, just the right age. Evidently this is better, more juicy, more tender.

The jury acknowledge that the boiled meat of this old horse is good, exempt from any particular flavor, being easily mistaken for beef of the second and third qualities, and appears to be a wholesome and agreeable article of food.

As side dishes, some Scotch collops, and a delicate chicken, with a white sauce, occupied the attention of the guests until the roast.

Roast horse.—This was a sirloin of the animal slightly pickled and larded. A burst of applause. Nothing could be better, finer, more tender—a sirloin of venison, which it resembles in flavor, is not superior. One member of the jury asks permission to carry off a piece. Several send up their plates for a second slice. The *gourmets* do not know yet the excellence of this morsel. I recommend it to them. It is perfect in every point. Ah, stomach, how ridiculous were your little prejudices!

The jury unanimously and enthusiastically declare that the sirloin of this old horse should rank with the most delicious meats.

M. H. Bouley feels himself conquered and convicted, and bravely confesses it.

But strict truth compels me to say that some excellent roast partridges which succeeded the roast were not absolutely overlooked by the jury any more than the *entremets* or the nuts and fruits of the dessert, which proves that the meat of the horse passes without trouble the difficult defiles of the digestive highway, a very interesting element in the discussion. For my part, remembering an aphorism both witty and true of one who perfectly understands the hygiene of the stomach, our colleague, M. L. Viron, I waited twenty-four hours before writing these lines in order to declare that I wrote without any remorse of digestion.

To sum up—the flesh of an old horse, of 28 years of age, gave us:

A superior soup.

A good and very eatable soup meat.

An exquisite roast.

Now is that not a very interesting experiment? Let us return to M. Renault who investigates this subject on account of the question of political economy which it involves; he will treat it calmly and scientifically, as he does everything,

without exaggeration or enthusiasm. Can we not already foresee a number of instances where the flesh of the horse which to-day is turned over to the soap boiler might be usefully employed as aliment? A vigorous young horse breaks his legs, dies of apoplexy, of colic or of any other accident, why not, since his flesh is good, healthy, agreeable and nourishing, let this animal enter into the general consumption? No doubt a stricter inspection would be necessary than for any other animal, for the horse is subject to a dreadful disease, one that is transmissible to man. It is true that M. Renault affirms that the cooked meat of a diseased horse loses all contagious qualities, and he is willing to prove it on himself. But then what danger for the butchers, the skinners, and even our own cooks.

But beyond these conditions of disease can we not foresee a period and that not far distant perhaps when the flesh of the horse shall be looked upon as a precious reserve in the great question of general alimentation? Already at Bruxelles there is a very well patronized butcher shop for horse-meat. One has been established for some time at Bavaria, another in Saxony, &c. &c. Even here at home, at Alfort, those who live near the school, since they have tasted horse-meat, every time that a horse is killed and that the director of the school has given it as his opinion that the flesh is fit for food, the neighbors, I say, hasten to secure a piece, and of the animal of which I yesterday had my share not one particle remains. And really I declare that they are not to be pitied who have consumed him. Has it not often happened in the rough chances of war that an army in want of provisions has been obliged to kill and eat their horses? A dire extremity without doubt, but one which, in a hygienic point of view, has no serious consequence, and that is the essential point for the time being.

It is doubtful however, as long as there are beeves, cows and sheep, whether the flesh of the horse will become an element of general consumption. The horse is too useful to man, he is too much his friend, companion, his fellow laborer, his intelligent friend, for him to be taken from his natural calling and turned into a simple stall-fed animal. It will be probably only by accident or necessity that he can become food for man. But even those necessities are common enough to claim the attention of the public economist.

Whatever may happen, I must thank M. Renault for having dissipated a prejudice, silenced a repugnance, and taught me this new fact—that horse-flesh is good, wholesome and agreeable.

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BIBLIOGRAPHICAL RECORD.

I. *On the Organic Diseases and Functional Disorders of the Stomach.* By GEORGE BUDD, M. D. R. R. S. &c. &c. Philadelphia. 1856. Published by Blanchard & Lea. (Received from Geo. M. West.)

To all those who have studied "Budd on the Liver," this companion work which we have now before us will be doubly welcome. They have learned to appreciate the philosophical mind, the judicious experimentation and the practical common sense of the author; and a careful examination of this treatise on the stomach disorders will only add the more to their already favorable opinion.

The sixteen lectures which constitute the present work have already appeared in print, and are familiar to the English reader. They include the Croonian lectures for 1847, and are now republished with some additions by the author. We are happy to say that this reprint has no American editor.

We are satisfied that the student can find nowhere a better treatise on the organic and functional disorders of the stomach than these lectures of Dr. Budd afford. They include the treatment of congestion, inflammation acute and chronic, organic diseases, sympathetic disorders and indigestion, and dyspepsia in all its protean varieties.

A very interesting introductory lecture clearly displays the difficulties attending the study of stomach disorders, and is especially interesting for its accurate description of the modifications caused by the action of the gastric juice on the stomach *after death*, or what our author calls *self-digestion*. The structural alterations produced in the stomach by its own juice, have often been mistaken for the causes of death and the evidences of disease; and although the fact that the gastric juice did oftentimes exercise its solvent

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power after death upon the stomach which contained it, even so far as to dissolve large portions of that organ, was taught by John Hunter; yet the fact had been almost entirely neglected for many years, and many erroneous pathological statements have resulted from this cause.

"Softening of the intestines, like the softening of the stomach, caused by the action of the gastric juice, is occasionally met with in persons dying of phthisis or of typhoid fever; and it occurs from the same cause. The gastric juice passes in its active state from the stomach into the intestines, and not meeting with alkali enough to neutralize its acid, and thus destroy its solvent powers, it dissolves or digests the coats of the intestine after death."

The result, then, at which I arrive is, that the softening, with thinness, of the coats of the alimentary canal, described by Louis, the pasty or pulpy and the gelatiniform softening of Cruveilhier, and the other varieties, described by other authors, distinguished by the color of the softened tissues, are essentially the same change; and that this change, whether it exist in the lower end of the œsophagus, or in the great end of the stomach, or in the fore part of the stomach only, or in any part of the small or the large intestine, is the result of *digestion* after death,* like the softening of the great end of the stomach, remarked by Hunter, that occurs after sudden and violent death, in the midst of health, and soon after a meal.

I have occupied much of your time in considering the change, because I consider the subject is one of great importance. It is obviously important with reference to medico-legal enquiries; and it is important, also, with reference to the attainment of a right knowledge of the pathology of the digestive organs. Every one who has studied the diseases of these organs by means of morbid anatomy must have had his mind perplexed, as mine has often been, and his progress stayed, by the continually recurring question, What do these striking changes mean?

We would like to devote both time and space to a more extended notice of this valuable work, but we must close by

* The intestines are probably sometimes digested by their own proper juices, which, although alkaline, have been lately found to possess, like the gastric juice, a digestive power over albuminous tissues.

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an extract, which will give a ray of comfort to those who often suffer from "the remorse of a guilty stomach." Our author, after laying down general rules for the governance of all who desire to digest their food in peace, concludes with the following consolatory language:

We have now considered at some length the various disorders of digestion. It may be some comfort to persons who suffer from them to know that a stomach easily disordered may be the means of preserving them from more serious ills. Persons in the middle and upper classes of society who have strong stomachs generally abuse the gift—eat and drink more than the system requires; go on doing this for a time with seeming impunity; but pay, at length, the full penalty of their over-indulgence, in gout, or in some of the more serious ailments that result from the changes of structure in the different tissues which over-indulgence at table tends to bring on. The man whose stomach is easily upset is early taught to avoid such excesses, and so escapes the penalty. I believe it would be found that, in the middle and upper classes of society, a large proportion of those who attain an advanced age are in some degree dyspeptic.

II. *A Manual of the Practice of Medicine.* By George Hilaro Barlow, M. A. M. D. Cantab. With Additions. By D. Francis Condie, M. D. Philadelphia, 1856. Published by Blanchard & Lea. (Received from A. Morris.)

Dr. Barlow's work is one of a series of "medical manuals" published during the last two years by Churchill of London, which is now reprinted by the American publishers, Messrs. Blanchard & Lea, under the editorial supervision of Dr. Condie of Philadelphia.

The author, whilst he acknowledges the merits of the various eminent men who have preceded him in this department of medicine, especially alluding to Alison; the Principles of Medicine by Dr. Williams; Watson's Practice, and the unfinished labors of his colleagues, Drs. Bright and Addison, yet thinks "that we are at present in want of a

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hand book for students." There may be a necessity for some such production; but although Dr. Barlow's book is before us, we do not think the vacuum filled.

The fact is, that this is a common place book. It is neither good nor bad. It has all the insipidity of a neutral mixture; the caustic alkali and the pungent acid—neither predominates. In the beautiful and compact form of the Churchill edition, with the heavy linen paper and clear English type, it passed muster tolerably well; and although a little disposed to be sleepy, we looked over its pages without any extraordinary effort; but the American reprint has taken away much of its comeliness; and the additions of the American editor have added something to its dullness. The work, in its present form, is no longer a "hand book;" and few will prefer its six hundred pages of very correct but very dry matter to the eloquent productions of Stokes and Watson.

Dr. Condie's additional paper on cerebro spinal meningitis is worthy of attention as giving a good idea of that occasional epidemic disease; and his long article on yellow fever, closely imitated from La Roche's great work, is so much like its surroundings, that we could hardly distinguish author from editor, but for the brackets.

III. 1. *Book of Prescriptions, containing* 2,900 *Prescriptions, collected from the Practice of the most eminent Physicians and Surgeons, &c.* By Henry Beazley. Philadelphia. 1855. Published by Lindsay & Blakiston. (Received from Geo. M. West.)

2. *A Pocket Formulary and Physician's Manual.* By Thos. O. Powell of Sparta, Georgia. Savannah. 1855. (Received from J. W. Randolph.)

We cannot conscientiously say of any formulary and prescription book, "that this work should be found on the table of every physician;" for we believe if a medical man desires to lose all independence of opinion, all originality in prac-

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tice, and only aims to make himself a distributor of second-hand recipes and routine prescriptions, let him keep a prescription book on his table for constant reference, and he will surely accomplish his object.

Such compilations, however, are undoubtedly useful as suggestive oftentimes of therapeutical combinations, which have been adopted and tested by those whose larger experience and wiser judgment we ought to respect. Making their use the exception and not the rule, the country physician may improve greatly his armamentarium and increase his chances of success; and for this purpose, the excellent compilation of Mr. Beazley is probably the most complete and well selected work to be obtained in the language. Great care has been bestowed in the collection of recipes from all nations and languages, and good descriptions accompany each article of the materia medica. The book comprises a large amount of information, and is very neatly and compactly arranged, and printed by Messrs. Lindsay & Blakiston.

The Pocket Formulary, arranged by Dr. Powell of Georgia, claims no original merit, and only deserves to be regarded as a useful and convenient reference for the country practitioner. The prescriptions are, generally speaking, those which have been well tried and approved by the profession, whilst in addition we notice many memoranda on subjects apparently trivial, but which are often needed by those who practice their profession in remote and thinly populated districts. This little book has the well known face of Nathaniel Chapman as a frontispiece, and is bound stoutly in leather backs, adapting it for its place in the medical saddle bags of our southern and western brethren.

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IV. *Notice sur les Travaux de M. E. Brown-Séquard, D. M. P. Laureat de l'Académie des Sciences, Ex-Professeur de Physiologie au College Medical de Richmond, etc. etc.* 46 p. 4to. Paris, November 1855. (From Dr. Séquard.)

It is customary, we believe, for candidates for admission into the more distinguished of the learned societies of Europe, to make known what they have done in the field of science, and what the titles are on which their applications for honors are based. Such, we presume, is the object of this publication, which contains a succinct account of those results of his researches for which Dr. Séquard justly lays claim to originality. Some of the most important of recent advances in physiological science are included in the enumeration, which we trust will obtain for our former valued collaborator the vacant chair of Magendie.

V. 1. *Report of the Board of Trustees of the Massachusetts General Hospital.* 36 p. 8vo. Boston, 1856. (From Dr. W. J. Dale.)

2. *Second Annual Report of Inspectors of the Hospital at Rainsford Island.* Pamphlet, 20 p. 8vo. Boston. 1856.

The general hospital in Boston is a model institution so far as it goes, and the details of its management are interesting to all who desire that similar instruments of philanthropy may be established in other parts of our country.

During the last year, 915 patients were treated in this hospital; the mortality was one in nine; the average number of patients was 134. Relief was afforded, also, to 645 out-patients.

The revenue of this hospital is derived from the income on $180,000 of invested property, from pay-patients, and from subscriptions for free beds. Any one who pays a hundred dollars to the treasurer has a right to keep one bed occupied for one year. There were 24 such subscriptions last

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year. The whole number of free patients, last year, was 414; the number of patients paying board the whole or part of the time was 501. The average weekly expense of each patient was $5 64, about double the rate in the French hospitals. The total annual expenses were $43,252; the main items were: stores, $18,000; salaries and wages, $11,400; fuel, $3,700; medicine, $4,394.

Six physicians and six surgeons, an apothecary, a chemist, a microscopist, an admitting physician and four house pupils are attached to the hospital.

The report of the Rainsford Island hospital exhibits a pleasing picture of much good accomplished with a very small expenditure of money. This is mainly an emigrants' hospital, but affords an asylum, also, to any one who is sick and poor. It was opened two years ago; since then, its average number of patients has been nearly 200. The total cost of erecting buildings and providing for this large number of patients for two years, has been $56,000. Of this sum, $20,000 was expended in building; the expenses of fuel were $2,300; salaries and wages amounted to about $5,000.

A single physician, who is also superintendent, attends to the patients. He is assisted by a few medical students. The whole number of patients during the past year has been 868.

VI. *Transactions of the Sixth Annual Meeting of the Medical Society of North Carolina.* Held at Salisbury May 1855.

We have read with both pleasure and profit the report of the last meeting of the North Carolina medical society. As a Virginian, we feel a more than ordinary interest in everything emanating from our sister state, to whom we feel bound by ties of affection and respect; and it is therefore with pleasure we observe that the North Carolina profession are willing to devote a few days in each year to increase the reputation and add to the dignity of their calling.

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An examination of this pamphlet shows us that the society does not waste its time in the discussion of personal or sectional questions. The committees on practice of medicine and surgery promptly fulfilled their duties. An interesting address on auscultation and percussion was delivered by Dr. Pitman of Tarborough; a report from Dr. McKee of Raleigh, and a very good valedictory from Dr. Dickson of Wilmington occupied the three days of the session usefully and profitably, leaving ample time for many interesting debates on medical topics, whilst we doubt not that the discussion of a banquet, prepared in honor of the society by the medical gentlemen of Rowan county, was one of the most pleasant events of the session. These are evidences of the healthy condition of our brethren of the "old north state," which we are glad to recognize, and we cordially congratulate them on the apparent prosperity of their state organization.

EDITORIAL AND MISCELLANEOUS.

On Vaccine Virus.

The small-pox epidemic of the past winter having visited our entire Atlantic coast with great severity, has been more than usually prevalent in Virginia. It first appeared in Richmond county on the Potomac, early in October last. Shortly afterwards, the first case occurred in Richmond city; and during the winter, in various portions of the state, it has spread consternation and alarm throughout our rural population.

During this period, our vaccine agent, (who is required by law to furnish vaccine virus to a population of more than *one million five hundred thousand*,) was actively engaged in collecting vaccine scabs from every reliable quarter, in order to supply the extraordinary demand made upon him;

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and in the midst of these labors he was astonished to hear from citizens of New Kent county, that virus sent by him had *inoculated* those upon whom it was used with both varioloid and small-pox. The agent was not at first inclined to credit this statement, believing it possible that these cases might have occurred in the natural way; but soon, letters received from Caroline, Powhatan and Norfolk, convinced him of the sad truth of the charge; and he, whose office it was to protect the people from the ravages of this pestilence, unwittingly had aided in its propagation.

The agent at once, with characteristic frankness, laid these facts before the governor of Virginia. Mr. Wise, with his usual energy, appointed a physician, in whom he had confidence, to visit the sick of New Kent county, and ascertain if their disease was indeed small-pox or varioloid, and also to report whether the agent could be charged with any ignorance or mismanagement in the discharge of his duty. Dr. Patteson, the gentleman selected by the governor, performed the duty assigned him, and submitted to his excellency the report, which appears, by permission, in our present issue. The report of the special committee of the legislature is also published in this number; and our readers will perceive that the vaccine agent has been entirely justified by all the examining parties. It gives us pleasure to add, that even those who have been most afflicted by this lamentable occurrence, and who not unnaturally, in the moment of excitement and alarm, were violent in their condemnation, have now united with the executive and the committee in acquitting the vaccine agent of all censure.

The question, however, which most interests the profession is, "*How has this accident happened?*"

We might suppose it possible that the agent, in purchasing his virus, as he was compelled to do at times, might have been imposed upon by some fiend, who, for the sake of profit, might have sent him a small-pox instead of a vaccine scab. This conclusion is at once disproved by the report of Dr. Patteson. The inoculation with small-pox virus

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always causes *small-pox*, but the use of the agent's matter, wherever it produced mischief, has developed in the unvaccinated, "*small-pox, varioloid and apparent vaccine.*" The interesting paper of Dr. Parker shows that the use of virus *of his own rearing* resulted in similar consequences. Indeed, we see that the scab No. 15, plucked by Dr. Parker from a healthy child who never had been exposed to small-pox, caused varioloid in eleven unvaccinated persons, and confluent small-pox in two others.

We have then to accept the idea suggested by Dr. Patteson, that virus taken from the arm of a person who has small-pox incubating in the system at the time of vaccination, may be modified by the variolous poison, and rendered capable of producing apparent vaccine, varioloid or true small-pox.

This opinion is corroborated by the table of Dr. Parker, who certainly came into possession of this poisonous virus by some unknown process; who distributed it to various physicians, and who gave at one time two or three scabs to the vaccine agent. This table shows that his first vaccination was of two children *whose mother had small-pox.* According to the theory, the child's blood was tainted with the small-pox poison; the vaccine modified the original disease, and the incubating disease modified the vaccine scab.

This plausible theory may be answered by the enquiry, Why is it that this result has never occurred before? How often, in the history of vaccination, must it have happened that persons exposed to small-pox as were these children, have been vaccinated, and yet no bad consequences have resulted? Even with regard to this scab, we see that Dr. Parker gave a portion of it to a medical friend, who met with no ill consequences from its use. This same virus was used on many persons before its ill effects appeared, and it was from the arm of the healthy little girl who had no exposure to the disease, that the virulent matter was plucked.

When we examine the authorities on this subject, the

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theory of the modification of vaccine virus by small-pox is overwhelmed. We can only at present allude to a few experiments with regard to this point, on record.

Dr. Jewett of New Haven, Conn. (Boston Med. and Surg. Journal 1852,) makes the following statement:

"I was called a few weeks since to vaccinate a child who had been exposed for five days to the contagion from a very mild case of varioloid. I vaccinated the child on two successive days. Both of the vaccinations took well, and passed through their regular course, the child suffering but little except from the local affection. On the ninth day of the vaccination, the patient was taken with severe fever, which continued for about twenty-four hours, when she broke out with varioloid, and was quite sick for several days. The pustules were numerous, and were to be seen in the throat, nostrils and inside of the mouth. Was this true varioloid? If so, had it remained dormant in the system during the period occupied by the vaccine disease? *I would state that I have used the matter taken from the patient, in vaccinating several children twice, and have found it to be of good quality.*"

It is needless to point out the discrepancy between the proposed theory and the experience of Dr. Jewett.

In the London Lancet for 1830, the experience of Dr. Guillou at St. Pol de Leon (Dep. Finisterre) is to the same effect. This gentleman inoculated *six hundred persons* with matter taken from varioloid patients, not one of whom had small-pox.

When we examine the French authorities on this subject, we find that this question of the modifying influence of incubating small-pox on the vaccine virus has been decided in the negative, after a most elaborate series of experiments. The following extract, from the celebrated "Dictionaire en Trente," is very forcible:

The vaccine virus does not seem to ally itself with any other virus. When we inoculate with a mixture of vaccine nd variolous matter, only one of the diseases is generally produced; or if both are developed at once, they go through their stages, each one with its own proper characters.

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In a great number of experiments made by the committee of vaccine, or by their correspondents, they have taken vaccine matter from pustules placed intentionally in the midst of scrofulous ulcers, favus and other chronic complaints, and the vaccine virus was never contaminated.

Numerous experiments on this subject have been made by Dr. Taupin, at the Hospital des Enfans Malades. During the four years he spent in that hospital, he vaccined more than two thousand children in every condition of health, and laboring under every variety of disease; and he especially noted the result of the inoculation—and above all, if he could observe any modification of the vaccine virus taken from these subjects. M. Taupin says:*

"A great number of these children, laboring under measles, scarlatina, itch, varicella, *varioloid and small-pox*, have furnished vaccine matter which has never communicated either of these contagious diseases. The same result followed the use of vaccine virus taken from subjects afflicted with ricketts, scrofula, syphilis, tubercles, chronic eruptions, scald head, &c. &c. In no case (*we designedly insist on this point*) did the virus produce anything but vaccine."†

We have briefly submitted this interesting question to the reader, and hastily collected the arguments which may be urged on either side. The subject is one full of importance, and deserves our most attentive consideration.

Rice's Obstetric Tables.

We had designed, after translating the Obstetric Tables of Dr. Pajot, which appeared in our journal for February and March, to have attempted the arrangement of some additional tables on the management of labor, so that the series might be a complete epitome of the important points in the art of midwifery.

It has so happened, however, that Dr. Nathan P. Rice of

* Valleix quotes these experiments of Taupin as decisive *on this point*.

Husson, who wrote in 1803, declares that in small-pox patients, who were vaccinated after the eruption appeared, if matter was taken from the vaccine pustule, nothing but vaccine would follow. Aubert went farther, and declared that the vaccine pustule would modify the small-pox pustules, so that matter taken from any of the pustules would only produce vaccine.

† See Dictionaire de Médecine, Article Vaccine, p. 414.

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New York, himself a pupil of Pajot, was engaged in the same task with ourselves; and we owe to his kindness the opportunity of presenting our readers with the very carefully prepared charts which appear in the present number.

Dr. Rice has devoted much time and research to the arrangement of these tables, and a careful examination of their contents will convince the student of their completeness and utility.

The management of labor, both natural and unnatural, is treated with great conciseness and precision; and we gladly seize the opportunity afforded us of substituting his more accurate compilation for our own unfinished attempt.

Medical Society of Virginia.

THE seventh section of its constitution, as amended in 1853, assigns the *fourth Tuesday in April* for the annual meeting of the Medical Society of Virginia. We hope the country members will assemble in force, in Richmond, on the 22nd of this month, to celebrate the thirty-third anniversary of our state organization.

So far as we are informed, there is no unfinished business or constitution-tinkering to be apprehended; an eloquent address may be expected from our worthy president, Dr. Worsham of Dinwiddie; the remainder of the session may be exclusively devoted, if the fellows choose, to the discussion of subjects strictly appertaining to our science and art, or to the organization and elevation of our profession.

Physicians desiring to connect themselves with the Society, should address Dr. F. H. Deane, chairman of the committee on nominations. Gentlemen in arrears can forward the amount of their indebtedness to Dr. Charles E. Anderson. The librarian and curator, Dr. Peterfield Trent, will gladly receive pathological specimens of books from gentlemen who feel disposed to present them. Other communications should be transmitted to the corresponding secretary, Dr. Wm. D. Haskins.

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We may venture to remind chairmen of committees on scientific subjects that they have still time to arrange their reports. In looking over the list of standing committees appointed in 1851,* we observe the following, which, we believe, have never as yet furnished reports of any completeness: *On Surgery*, chairman, Dr. Hugh McGuire. *On Obstetrics*, Dr. Francis Deane. *On Chemistry*, Prof. Maupin. *On Practical Medicine*, Dr. Haskins. We sincerely hope that these delinquent fellows will furnish us at the ensuing meeting, with some observations, at least, on the subjects they are so competent to treat.

Without wishing to advert to the occurrences which have interrupted the usefulness of this association for nearly three years, we may observe that the path is now open again for the pursuit of its legitimate objects. Extraneous subjects are at last disposed of, and the profession may now harmoniously convene to compare and correct experience, to start new subjects of enquiry, to cement the bonds of kindness and fellowship, and to deliberate on the correction of abuses and the introduction of useful reforms.

VARIETIES.

Medical News and Items.

ASTLEY COOPER PRIZE.—The sixth triennial prize of three hundred pounds, ($1,500) founded by Sir Astley Cooper, will be adjudged to the author of the best essay or treatise *on the structure and functions of the thyroid gland.*

The conditions expressed by the illustrious testator, in relation to these prizes, were: that the rewarded essays "should contain original and unpublished observations, and should be illustrated, as far as the subject would permit, by anatomical preparations and drawings, which, as well as the essay itself, should become the property of Guy's hospital." This prize is open to medical men of all countries. The essays, in the English language or with an English translation, should be sent to the physicians and surgeons of Guy's hospital, before January 1, 1859, with the author's name in a sealed envelope. The unsuccessful essays, with the preparations accompanying them, will be placed in the museum of Guy's, at the disposal of their owners.

* See Stethoscope, vol. 1, p. 456.

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VISIT OF FOREIGN SAVANS.—It is understood that the meeting of the American association for the advancement of science, which takes place in August next at Albany, will be attended by no less a personage than Baron Liebig. Other scientific big wigs have been invited from abroad, among whom we note particularly Leverrier, Struves and Mr. Airy. The presence of the astronomers is especially desired, because, on this occasion, the Albany Observatory is to be inaugurated.

MONEY IN AND OUT OF THE PROFESSION.—In an interesting address delivered by Dr. Simpson recently to the newly elected graduates of the Edinburgh university, he advised the M. D.s not to expect to make as much money as if embarked in trade; or in any other profession besides the medical, but cited the following instances of the "ups and downs" of great men's lives. Dr. Hall, in the Crimea, has a salary of two guineas a day, "an income far less than that made by many a second or third rate village apothecary (general practitioner) in England;" Dr. Cheyne of Dublin, when 34 years of age, made only three guineas during the first half year of his second years' practice—nine years subsequently he was making £5,000 annually; Dr. Chambers in his fifth year of practice, and when 34 years of age, made £211 in fees—seventeen years subsequently, £9,000 annually; Dr. Baille £11,000 in one year; Dr. William Hunter had not money to advertise his lectures. In 1788, the son of an English clergyman attended the classes of Edinburgh, and lived in a room which cost him 6s. 6d. a week; in after life, as Sir Astley Cooper, his professional income in one single year amounted to £23,000. We might add to these the following items, stated on good authority. Mr. Colles of Dublin made £11,000 a year; Albert Smith, a surgeon, gave up his profession in dismay, and made £19,000 the next winter out of "Mont Blanc;" Professor Anderson, now in London, makes something like £10,000 a month.

OLD AGE.—Said Effendi, jester to the sultan, and who has held the same post under four Ottoman emperors, has just died at the age of 120 years and 7 months. Notwithstanding his extreme age, he retained, it is said, the use of all his faculties to the last.

DR. BROWN-SEQUARD has been presented by the Academie des Sciences of Paris with the prize of "Experimental Physiology" for his researches on the "Physiology of the Spinal Cord."

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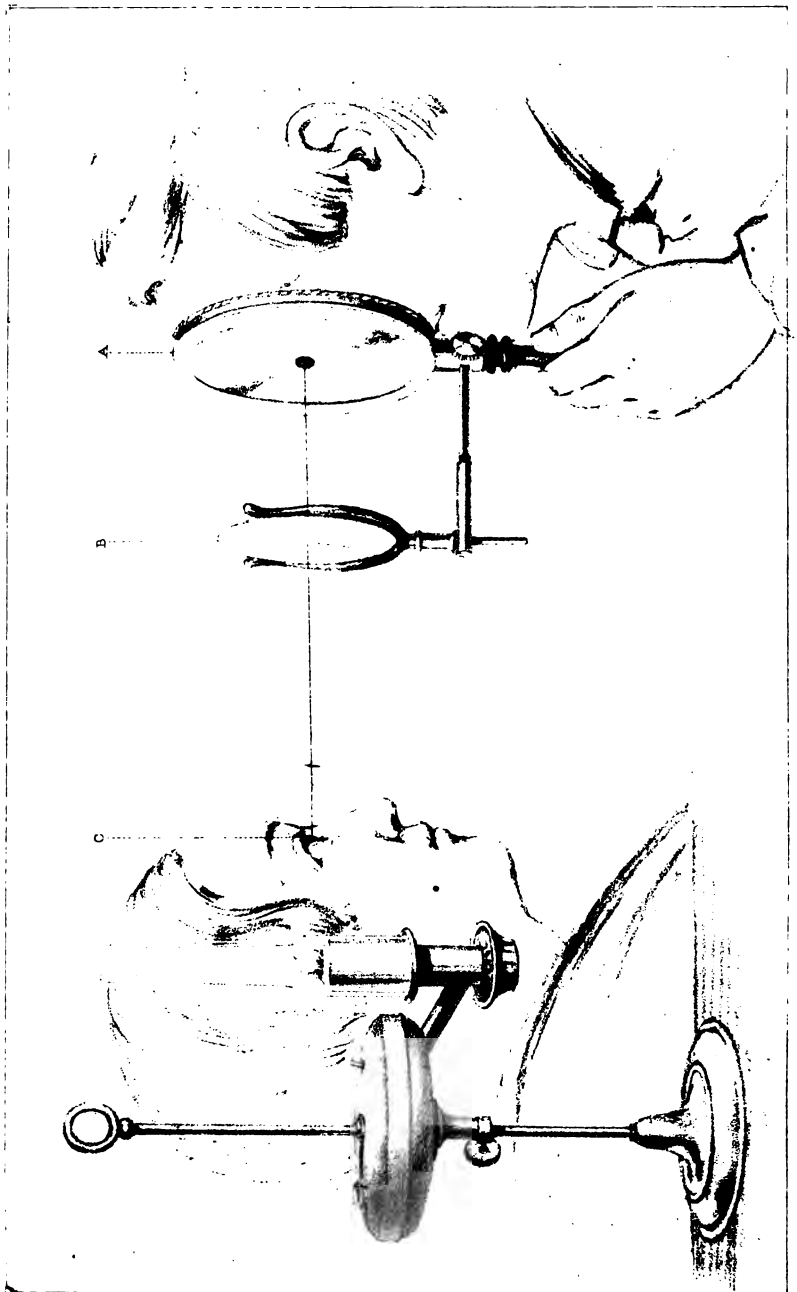

Ophthalmoscope modified from those of Anagnostakis and Jäger.

Lith. Press of Ritchie & Dunnavant, Richmond, Va.

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VIRGINIA MEDICAL JOURNAL.

THE STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL

COMBINED.

MAY 1856.

ART. I.—*On the Ophthalmoscope—its Uses and Method of Application.* By JOHN H. DIX, M. D. Boston, Massachusetts.

IT was my intention to communicate to your journal notes and illustrations of the most striking and distinctive cases only of a long series of ophthalmoscopic observations. In deference, however, to your suggestion, that at present little is to be found explanatory of this discovery except in transatlantic publications, I shall, at the risk of repeating what is to some of your readers quite familiar, preface my future papers with one upon the origin and application of the instrument, of one of the many forms of which you have the accompanying drawing.

By means of it, the fundus of the globe and all the space between it and the iris are as clearly submitted to ocular inspection through the dilated pupil as are the anterior textures to the unassisted eye, allowance being made for the refracting influence of the transparent media of the eye which is examined.

The first suggestion pointing towards it was made by M. Prevost in the year 1810. He demonstrated that the lumi-

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nous appearance from the pupil of many inferior animals never is seen in total darkness; is not at all the result of passion or volition, and depends wholly upon the reflection of light which has entered the eye from without.* I have not seen his statements in full, but they are quoted and alluded to by many later writers on this subject, and are to be found in the Bibliothéque Britannique, t. 19th.

A similar luminous appearance has also been observed in the human eye. I remember certainly, some ten years ago, seeing a very decided red glare from the pupils of a lady with whom I was conversing. I mentioned it to her, and she informed me that she was excessively near-sighted. Her pupils were very large. That the myopy in this lady was not owing to amaurosis, is proved by her sight continuing in the same condition as at that time.

This appearance was observed by Beer as early as 1792, in some cases of amaurosis, and was thought by him to be diagnostic of a peculiar form of this disease, to which he gives the name of "cat's eye" (katzenaugen.)†

The first attempt to obtain, by artificial aids, from an eye in its normal condition, this luminous reflection, was made in 1846, by Mr. Wm. Cumming; and in as much as his experiments naturally led the way to the multiplied and improved contrivances by which much more than he attempted is now accomplished, I quote from vol. 29th, p. 284 of the Medico Chirurgical Transactions, as follows:

"The object of the present paper is to show that the healthy human eye is equally or nearly equally luminous as the eye of the cat, dog, &c. when observed under favorable circumstances, and the application of the abnormal appearance or want of this luminosity, to the detection of changes in the retina and posterior part of the eye.

* Gruethuisen, Rudolphi and Müeller have instituted similar investigations with the same result. Ruete Krankheiten des menschlichen Auges. 1st Leiferung, p. 26.

† Lehre der Augenkrankheiten, band ii. Wien. 1792. This glare is especially easy of observation in cases of congenital absence of the iris. A case of this kind is related by Behr in Hecker's Annalen, 1839.

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"The reflection from the posterior part of the human eye may be seen in the following manner: Let the person whose eye is to be examined, be placed at the distance of ten or twelve feet from a gas or other bright light; the rays of light must fall directly on his face; all rays passing laterally of his head must be intercepted by a screen, placed half way between the light and the eye examined. If the reflection be bright, it will be at once seen from any spot between the light and the screen. The following observations were made in two rooms; in one of which was a gas light, the other completely darkened. The person whose eye was to be viewed was placed in the dark room, five feet from a half closed door opening into this room; he directly faced the light, also at the distance of four or five feet from the door.

"The appearance of the reflection was in most cases extremely brilliant, when seen from a position between the door and light. In some it was at once obvious, with the door wide open; in others, it was seen with great difficulty, and not till every ray of light passing to the side of the iris was carefully intercepted by the door on one side and the hand or a book on the other. The reflection was always seen much more readily and brilliantly when the eye was turned slightly to the side, and the rays of light passed through the pupil obliquely. On passing to the other side of the door, the luminosity was seen with greater difficulty. In this position, it is necessary to have the eye turned to the side, to exclude all rays by the hand, except those passing directly to the eye. In this way the reflection may be seen distinctly at the distance of eight inches.

"In the majority of cases, however, it may be seen as follows: Let the person under examination sit or stand eight or ten feet from a gas light, looking a little to the side; standing near the gas light, we have only to approach as near as possible to the direct line between it and the eye to be viewed, at once to see the reflection. Or, in a dark room, a candle being placed four or five feet from the eye, if we approach the direct line between them, we shall be able at once to see it in many cases. If solar light be admitted through a nearly closed shutter into a dark room, the luminosity may be seen when the pupil is tolerably dilated, the patient standing five or six feet from the aperture, and the observer occupying the position before indicated.

"These, then, are the circumstances necessary for seeing the luminosity. *a.* That the eye must be at some distance from the source of light; the distance being greater in proportion to the intensity. *b.* That the rays of light diffused

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around the patient (and sometimes around the eye itself) should be excluded. *c.* That the observer should occupy a position as near as possible to the direct line between the source of light and the eye examined; hence it is sometimes necessary for the observer to stand obliquely, that his eye may approach nearer to the direct line.

"The appearance of the reflection itself not only varies much in color and intensity in different persons, but also from the circumstances under which it is seen, viz: the greater or less intensity of light, the position of the eye examined, and the distance at which it is viewed.

"When the reflection is seen under the influence of a dim light, as that from a candle, or a few solar rays, a red lurid glare, like that from a dull coal fire, is observed, evidently proceeding from the bottom of the eye, and though not distinctly concave, yet conveying the idea of concavity. The character of the reflection thus seen by a faint light, at the distance of two or three feet, is very uniform, and does not present much variety of tint.

"When the eye receives rays from a good bright light ten feet distant, and we stand near the light, the reflection is then seen extremely brilliant, presenting a fine metallic lustre, and varying from a bright silver or golden, to a decided red tint—the latter being the more usual color. While viewing the reflection at this distance, it sometimes undergoes a distinct change, suddenly altering from a copper or red color to a silver tint. This happens sometimes in consequence of a slight movement of the eye, but not unfrequently is observed without any movement having taken place.

"Although the reflection is more readily seen in an eye with a large pupil, its lustre does not depend upon this circumstance. In two eyes, with pupils of equal diameter, the intensity of the reflection frequently varied greatly. In one case, in which the reflection was very dusky in appearance, and the pupil small, atropine was dropped into the eye. I then observed that, though the extent of luminosity was increased, it still retained the same dusky hue. The greater facility with which the reflection is seen when the eye is directed slightly away from the light, appears to depend on the more patulous condition of the pupil.

"On approaching within a few inches of the eye, the reflection is not visible, for, before our eye can be brought within range of the reflected rays, the incident rays of light are excluded. On placing before the eye examined a black card with an aperture the size of the iris, the intensity of the re-

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flection was observed to be somewhat diminished. In cases in which the lens had been removed, the reflection was indistinct at a distance, but was rendered somewhat clearer by the aid of a double convex lens placed before the eye examined; but at two or three feet distant, the reflection was as obvious as in cases in which the lens was present."

The experiments of Mr. Cumming were, in 1847, followed by those of M. Brücke, professor of physiology at Vienna, which may be considered to be another step towards the ophthalmoscope, as it now is. The method of M. Brücke* is this: The surgeon holding in one hand a wax candle, places himself at the distance of eight or ten feet in front of the patient, the flame of the candle being just below the level of the eye to be examined. With the other hand the surgeon holds between his own eyes and the light a screen, the upper edge of which should be exactly even with the summit of the flame, in order to protect his own eyes from the direct light, while at the same time he looks over the edge of the screen into the pupil of the patient. The patient, with the eyelids widely opened, should now look at the most distant objects in front of him, changing from one to another near it, or else gently move the eyes until the pupil presents to the observer a bright red color, and the iris has a greenish tinge. If the patient looks at the flame, this reflection is lost.

In 1851, an instrument called an orthoscope, by Dr. Czermack, was described in the Prager Vierteljahrschrift fur pract Heilhunde, vol. 4th, the intention of which was to "prevent as much as possible the refraction and deviation of the rays of light by the anterior surface of the cornea. This was accomplished by putting the eye under water, the refractive power of which is very nearly the same as that of the cornea and aqueous humor. It appears to have been used in the clinique of Professor Arlt, who availed himself of a small gutta percha case with a plane glass front, which easily fitted upon the face, and prevented the running out of

*Leuchter der menschlichen Augen. Müeller's Archives, 1849.

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the water. By this means much of the refracting agency of the transparent media is obviated, and a view of the interior facilitated. The possibility of this was suggested still earlier, (Le Cat. traité des sens. Amst. 1744, p. 174.) The practical inconveniences must have prevented the general use of the orthoscope, even if it had not been superseded by the ophthalmoscope. This device, the orthoscope, seems to have been foreshadowed more than a hundred years ago by Mery, who, while holding a cat under water, saw "the color of the bottom of the eye and the blood vessels ramifying thereon."

Five years later this was verified by Lattêre, who demonstrated that the refracting power of the cornea is neutralized by covering it with water.*

Mr. T. Wharton Jones, in an article dated October 1854,† states that seven years before, a Mr. Babbage showed him a model of an instrument contrived for looking into the interior of the eye. It consisted of a bit of plane mirror with the silvering scraped off at two or three small spots in the middle, fixed within a tube at such an angle, that the rays of light, falling on it through an opening in the side of the tube, were reflected into the eye to be observed, and to which the one end of the tube was directed. The observer looked through the clear spots of the mirror.

To M. Helmholtz, professor of physiology in the university at Hoenigsberg, is due the credit of first devising and bringing fully to the notice of the profession, an apparatus expressly designed, and in some cases competent for a distinct view of the internal textures of the eye. It is called by M. Helmholtz an augenspiegel, (eye mirror,) is very complex in its construction, inconvenient for application, and when properly applied, less effective than any one I have had the opportunity of using. The peculiarity which (its

* Der Augenspiegel, seine Anwendung und Modificationen nebst Beiträgen zur Diagnostik inmerer Augenkrankheiten von Dr. Van Tright. Aus dem Holländischen mit Lusätzen bearbeitet von Dr. C. H. Schauenberg, Docenten an der Universität zu Bonn, 1854.

† British and Foreign Med. Chir. Rev.

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clumsiness excepted) distinguishes it from those now most employed is, that the light is reflected back to the eye of the patient from a plane transparent glass mirror composed of three laminæ placed obliquely in front of a chamber, the inner surface of which is blackened. Through this chamber and mirror the observer looks into the eye of the patient, unavailingly, however, except as to the crystalline lens, and perhaps the anterior portion of the vitreous humor, because, although the light is polarized and partially reflected, many of its rays are absorbed and not reflected, while the triple glass mirror itself obstructs the vision of the observer. One is obliged also with this instrument to look through a concave lens, except only when the eye he is looking at is extremely myopic.*

To this instrument Dr. E. Jaeger gave greater effect by substituting for the plain transparent mirror a concave silvered one, from the centre of which a small circular portion of the amalgam has been removed.

To this instrument, the chief objection is the distance from the eye of the observer to the mirror itself, by which the field of vision is very much diminished, and its great advantage, the intensifying of the light by reason of the concavity and silvering of the mirror.†

It has also the convenience of enabling the observer to dispense with the screen which is required with the apparatus of M. Helmholtz and others.

In 1853 or 1852,‡ M. Coccius of Leipsic very essentially simplified and improved upon the original ophthalmoscope of Helmholtz, by substituting for the transparent triple mirror, a silvered mirror perforated centrally with a hole corres-

* The ophthalmoscope of Helmholtz is of interest as the earliest, and is minutely described in the Beschreibung eines Augenspiegels zur Untersuchung der Schutzhaut im menschlichen Auge von Helmholtz mit 1 Kupfertafel. Berlin. 1851. An abstract of this will be found in No. 26 of Braithwaite's Retrospect, taken from the Monthly Journal of Med. Science, July 1852.

† Staar und Staarenoperationen nebst anderen Beobachtungen und Erfahrungen. Wien. 1854.

‡ Med. Times and Gazette, Sept. 10th, 1853.

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ponding to the pupil; by dispensing altogether with the darkened chamber between the eye of the observer and the mirror, and by placing obliquely between the lamp and the mirror a convex lens to concentrate the light upon the mirror. By this instrument a much stronger light is thrown upon the retina, an obstruction removed, and a wider range given to the eye of the observer. In the practical application of the ophthalmoscopes of Helmholtz and Coccius, attention must be paid to the following difference: With the ophthalmoscope of Coccius, the mirror is to be placed in the same position as with that of Helmholtz, but the lens must be so placed between the mirror and the lamp that the light, which has been concentrated by the lens upon the mirror, shall, when reflected from it, form upon the face a square of reflection (viereckigen schien) in the centre of which is a black spot, which black spot must, during the examination, fall in the centre of the pupil; while with the mirror of Helmholtz is obtained only a small clear image of a flame (helle Flammenbildchen.)*

Lastly, in 1854, Dr. Anagnostakis of Athens divested the instrument of all its unnecessary appendages, rendering it extremely simple, sufficiently effective for a full examination of the fundus of the globe in most cases, and immediately available to the most inexperienced observer.

It was introduced by him at the clinique of M. Desmarres in Paris, and has since been very extensively adopted upon the continent and in Great Britain.

Dr. Anagnostakis has been accused of claiming as his own original invention this simplified ophthalmoscope, of which an example had been already shown to him at the clinique of Dr. Graefe of Berlin, as the invention of Professor Coccius of Leipsic. But I have before me an engraving, issued at Leipsic by Prof. Coccius himself, of his own ophthalmoscope, which has all the clumsiness of that of Helmholtz, and is rendered more effective as to its illu-

* Dr. Adolph Coccius über die Anwendung des Augenspiegels nebst Angabe eines neuen instruments mit einer Tafel. Leipzig.

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minating power only by the interposition of a convex lens between the plane mirror and the lamp.

From his own statement,* he appears to have entertained the idea that instead of the plane mirror and convex lens, a concave mirror might be used, but finally to have satisfied himself theoretically that the combination of the plane mirror and convex lens is to be preferred. I conceive, therefore, that to Dr. Anagnostakis belongs at least the credit of demonstrating the superiority of the instrument which he claims as his own. I translate his description of it as follows: †

My ophthalmoscope consists simply of a round concave mirror, having a diameter of two inches, and a focal distance of four and a half inches, the silvered surface being protected by a blackened copper plate. The centre of this mirror is perforated in a round flaring (evasée) hole of a diameter of one-quarter of an inch.‡ A small handle completes the instrument.

Now, strictly speaking, for an ophthalmoscope two conditions only are required, and they are:

1st. To throw into the fundus of the globe a large amount of light.

2d. To give the observer an opportunity to look directly into the bottom of the globe thus illuminated, and obtain a clearly defined view of the surfaces presented.

Both of these conditions are fulfilled by this instrument.

* In a letter to Dr. C. Bader he says, "In the year 1853, I drew a parallel between the advantages resulting from the use of a slightly concave and perfectly plane mirror joined with a convex lens. I preferred the latter, and have shown (p. 31, in a book upon the ophthalmoscope *opuscule remarquable*) that optically both forms of mirror are alike, but that the plane mirror joined with a convex lens furnishes more advantages in an examination near the eye." Brit. and For. Med. Chir. Rev. April 1855. From which I infer, not that Prof. Coccius ever used or advised a concave mirror, but that he thought of it.

J. H. D.

† Essaé sur l'exploration de la Retine et des milieux de l'œil sur le vivant au moyen d'un nouvel ophthalmoscope par A. Anagnostakis Docteur en Médecine de la Faculté d'Athénes. Paris, 1854.

‡ At the Royal London Ophthalmic Hospital, Moorfields, an ophthalmoscope is preferred, the central aperture of which is smaller, and made by scratching off the silvering, leaving the glass intact, and the focal distance of which is six inches. Brit. and For. Med. Chir. Rev. April 1855.

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1. *The illumination.*—It is unnecessary to demonstrate that such a mirror must have a very great illuminating power, as every body is familiar with the fact that light is concentrated by concave mirrors.

It might at first thought be supposed that the concentration of the light from the mirror upon the eye of the patient could be obtained only by placing the mirror at a certain invariable distance, and that therefore, if the observer changed the distance between his own eye and that of the patient, he would be compelled to take another mirror of a different focal distance. It is not so. With concave mirrors the length of the conjugate focus is in the inverse ratio of the distance between the mirror and the point of illumination.

The nearer the lamp approaches to the mirror, the more the focal distance is elongated, and the reverse. If then the observer is obliged to recede from the eye at which he is looking, he has only to bring the lamp nearer to himself in the ratio of his increased distance from the eye. He will of course place the lamp farther off if he wishes to look nearer.*

A few trials are required to find the proper distance, and one is guided to it by the gradual increase or lessening of the reflection. It is accomplished when there is thrown upon the cornea a small oblong brilliant reflection, which is the inverted image of the flame.

The focal distance of the mirror being thus variable at pleasure, this instrument accommodates itself to the examination of either myopic or presbyopic eyes, and even of eyes wanting the crystalline lens, since it admits of being used at various distances.

Nor is it necessary, for a successful exploration, that the reversed image of the flame should be presented upon the cornea as accurately as has now been supposed. Provided that the fundus of the globe is distinctly seen, the reflection is of course sufficiently accurate.

A screen is not required with this instrument.

2. *The perception of objects.*—It is evident that from so strong an illumination should be obtained the clearest possible view of objects. As the eye of the observer is placed close behind the central hole of the mirror, the field of vision is enlarged as he recedes from the eye at which he is looking. The flaring, cupshaped (evasée) form of the whole allows one to look obliquely.

* The crossing of the rays proceeding on all sides from the oblique surface of the mirror prevent any apparent shadow resulting from the central aperture.

J. H. D.

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Both before and since the introduction of the ophthalmoscope of Dr. Anagnostakis, many others have been brought forward under different names, although in the preceding sketch I have restricted myself to the mention of the few who seem to me to have essentially contributed to bring the instrument to its present condition of simplicity, general applicability* and effectiveness.

Its simplicity is its most striking and commendable characteristic, especially as compared with the earlier inventions which were so complex as to be almost indescribable.† It is generally applicable, and in all cases effective, because, while with an argand or solar burner the illuminating power fully suffices for the deeper textures, it is adapted to the crystalline lens and adjacent tissues by merely using a smaller flame. To the instrument of Dr. Anagnostakis I find but one objection, and that arising very naturally from his desire to simplify. This objection is the want of any contrivance for placing a lens between the mirror and the eye of the patient. The use of a convex lens in examining a presbyopic, or of a concave lens in examining a myopic eye, is often required by an observer whose own vision is normal, and yet, unless an assistant is present, one hand may be required to raise the lid of the patient while the other holds the ophthalmoscope.

To meet this want I have caused to be appended to the

* The following is a list of some of the gentlemen who have invented, improved or altered this instrument: Mr. Babbage of England, 1847; M. Helmholtz, Berlin, 1851; Dr. C. G. T. Ruete, Leipsic, 1852; MM. Follin et Natchet, Paris, 1852; Prof. Adolph Coccius, Leipsic, 1853; M. Epkens, an instrument maker at Amsterdam, 1853; Dr. Edward Jaeger, Vienna, 1854; Dr. A. Anagnostakis of Athens, 1854; Dr. E. Williams, Cincinnati, U. S. and Mr. J. Dixon, London, Dr. R. Ulrich of Gottingen, 1853; Inspector Myenstein, Gottingen, 1854; Dr. Graefe, Berlin, 1853. Most of the above named have written upon this subject, and in addition to them T. Wharton Jones, W. R. Sanders, W. Mackenzie, C. Bader, and Bransby Roberts, T. Spencer Welles, F. C. Donders and M. Van Gondever of Utrecht, Dr. Van Tright of Holland, translated into German by C. H. Schauenberg of Bonn, Dr. Kussmaul of Heidelburg, &c.

† The earliest detailed description of Helmholtz's ophthalmoscope I put into the hands of a very ingenious optician, with a request that he would make one. Six months later, he replied that it was wholly unintelligible.

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mirror of Dr. Anagnostakis a support for a lens similar to that of Dr. Jaeger, but differing from it in allowing the distance between the mirror and lens to be varied at pleasure.*

The instrument, as represented in the accompanying drawing, is in fact nearly identical with that now employed by Dr. Jaeger at Vienna, but different in this very important respect, that the mirror had nearly double the area of Dr. Jaeger's, and in consequence a much greater illuminating power. It is also a screen for the eye of the observer, which that of Dr. J.'s is not.

The drawing hardly requires explanation.

The mirror A is reduced one-third in diameter. The observer holding its handle in his left hand and placing his eye immediately behind the mirror, looks through its central aperture and also through the lens B, into the pupil of the eye of the patient C.

The flame of the lamp should generally be a little in advance of and quite near to the ear of the patient, and always in the same plane with the eye of the patient and of the observer. The distance at which the mirror is to be placed from the eye of the patient depends upon the focal distance of the mirror itself and the refracting powers of the eye examined and the lens used. With the mirror of Anagnostakis it varies from three and a half to five inches.

Every thing being arranged in this way, and the eye of the patient looking forward, a reflection from the mirror will be seen upon the cornea. By varying a little the distance of the mirror, it will be seen where the strongest, best defined reflection is obtained. A still slighter change of position will now bring from the fundus of the eye a red glare, and immediately, with or without another slight movement, the different textures of the retina will be in view.

For examination of the crystalline lens a wax candle suf-

* It is the contrivance of Mr. Wightman, philosophical instrument maker, Cornhill.

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fices. For the deeper textures an argand burner, or which is better, a solar burner, from which the light is stronger and whiter.*

I have also appended to my lamp behind the flame a concave mirror of about six inches diameter and ten inch focus, by which the light may be concentrated upon the mirror of the ophthalmoscope, and a still more vivid illumination obtained. This arrangement is needed only when the examination of the retina or vitreous humor is to be made behind a partially opaque lens or capsule, for which purpose it is not possible to obtain too much light.

This concave mirror behind the lamp has of course the same effect in intensifying the light of the lamp upon the mirror of the ophthalmoscope that a convex lens has before the lamp. With a plane ophthalmoscopic mirror, this concave mirror behind the lamp must give the same result much more conveniently than it is accomplished by Coccius and others through the means of a lens attached to the mirror of the ophthalmoscope.

A very satisfactory ophthalmoscope can always be extemporized by scratching off from near the centre of a fragment of common looking glass a little of the silvering. For the examination of a myopic eye, this often suffices very well. A presbyopic eye requires either a concave mirror behind the lamp or a convex lens between the lamp and the bit of looking glass, and possibly also between the looking glass and the eye of the patient another convex lens, for the refracting power of a presbyopic eye is often so little, that without some compensating arrangement externally to it, the observer is compelled to recede too far from it for distinct vision.

Moreover, the retina being usually seen through the refracting magnifying influence of the crystalline lens, an eye which has lost the lens will certainly require, with the most

* To obviate the effect of the red and yellow rays, and approximate the light to that of day, Mr. James Dixon of London and Dr. W. Mackenzie of Glasgow, advise a glass chimney of a bluish tinge.

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effective ophthalmoscope, the interposition of a convex lens, unless the other transparent media have an unusually high refracting power.

The pupil of the eye to be examined must have been dilated previously, and for this purpose atropine is much the most convenient and effective agent. The examination should be made in a darkened room, free from reflecting surfaces.

It is also directed, by some writers,* that a horizontal tablet be placed behind and above the head of the observer, by looking at the divisions of which, the patient is to change the position of his eye and bring successively each portion of the retina under the view of the observer. Now, to most eyes which are examined in this way, the tablet must be invisible, and when the patient cannot, as in most cases he can, change, at will, the position of his eye, it is possible for the observer, by a little change in the position of his eye and ophthalmoscope, to get a view of different parts of the retina with sufficient distinctness.

But why is not the fundus of the eye visible without an ophthalmoscope? Through a dilated pupil a large amount of light is received upon the retina; and moreover, the interior of the globe is otherwise partially illuminated. According to experiments by Volkmann, light is admitted into the eye through the sclerotic, iris and ciliary processes. As to the sclerotic, this is demonstrable by concentrating upon it the diffused light of day by means of a large convex lens. It would seem, therefore, that the reflecting surfaces of the place of entrance of the optic nerve, of the red vessels and of the retina itself, should be ordinarily visible.

That they are not so, is due to the refraction of the transparent media. If from a certain point without an eye rays of light pass into it, so that passing through the refracting media, they meet again at one point upon the retina, these rays, when reflected must, in passing out of the eye, be

* Helmholtz and Coccius.

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refracted precisely as in entering it, and can meet nowhere outside of the eye but at the point of departure.

Now, in order to see the retina of the eye of a patient, the observer must put his own eye with reference to the patient's eye, in the same position as the visible point just spoken of. But if the observer place his eye in this position, which must be in front of the pupil, his head necessarily intercepts nearly all of the light which through the pupil could reach the portion of retina, towards which his vision is directed. Therefore, unless from the eye of the observer enough of illuminating rays can be thrown, the retina of the patient's eye is in shadow and the pupil appears black.

Now, the ophthalmoscope does give to the eye of the observer, and its immediate surroundings, a strong illuminating power, so that the observer sees in the eye of the patient something else than the reflection of his own pupil. Thus is explained the blackness of the pupil, when looked at from in front. But if we look obliquely at a pupil, it still for the most part appears black, and this because we are then looking at the anterior, lateral portions of the retina, which are cast into shadow by the iris and ciliary processes.

Some scattering light, however, from the fundus, does in that position reach the eye of the observer, and always imparts to a pupil, obliquely viewed, a light or hazy aspect, which I do not doubt has sometimes given occasion for mistaken diagnosis of cataract. It is only when obliquely viewed that the glare sometimes seen from the eyes of the inferior animals and of man becomes vivid, and it will be remembered that in the experiment of M. Brücke already detailed, the reflection is obtained only when the eye is looked at obliquely. The explanation of this has been more briefly given by Dr. C. G. T. Ruete, when he says that "looking into the eye of another, we see reflected only the blackness of our own pupil."*

*Bildliche Darstellung der Krankheiten des menschlichen Auges. Leipzig, b. 1, p. 24.

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If the foregoing explanation is not understood, it will readily be so on looking at a diagram given by Dr. Anagnostakis,* and copied by others.†

Caution in the use of the ophthalmoscope.—Its present value as a means of diagnosis, no one familiar with it can deny. Its prospective, therapeutic, practical value, cannot be doubted by any one who admits, that advantage has been derived in the treatment of pulmonary and cardiac diseases from stethoscopic and post-mortem investigation, or who believes with Van Tright that the "*Spes therapiæ* is to be looked for only in the differential diagnosis of morbid processes and states." But if injudiciously used, it may do harm. Mr. T. Wharton Jones, in his summary of ophthalmoscopic contrivances, for the compilation of which he certainly merits the thanks of the profession, approaches, but does not touch upon, this subject where he says, "the use of the ophthalmoscope is necessarily circumscribed by the capacity of the eye to bear the concentrated light."‡ But how is this capacity to be measured?

If an eye is partially amaurotic, or, which is at present substantially equivalent to it, vision is impaired or disturbed, without any obvious§ opacity of the transparent media, when and how may the ophthalmoscope be used? The liability of astronomers, sea captains and mates, microscopists, engravers, watchmakers, &c. to amaurosis, is well known. They are exposed to or make use of either reflected or refracted natural and artificial light.

In the ophthalmoscope artificial light alone is used, and it is always reflected. If it is reflected only as in the ophthalmoscope of Helmholtz, it is ineffective and useless in most cases for exploration of the posterior hemisphere, and for

* Essai in l'exploration de la retine, &c. 1854, p. 12.

† Med. Times and Gazette July 1st, 1854, p. 7. Braithwaite's Retrospect, Jan. 1855, p. 179.

Beitrage zur Pathologie des Auges, von Dr. Edouard Jaeger. Wein. 1855, p. 7.

‡ Med. Chir. Rev. Oct. 1854.

§ To the eye with or without a lens, or by the method of M. Sanson.

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determining the state of the lens, not more decisive than the ordinary catoptric method. If the light is, besides being reflected, refracted and concentrated as in the later improvements, the instrument is unquestionably effective for examination of the posterior hemisphere, and if incautiously used when vision is not much impaired, may be considered prejudicial very nearly in proportion as it is effective.*

Still it is very desirable to resort to it in some cases of incomplete amaurosis; but in doing so, the following should be in general excepted:

First—All cases of incomplete amaurosis which are of recent date, in which vision is but little affected, and which were preceded or at their commencement accompanied by severe pain in or near the eye, by intolerance of light, or by tenderness of the globe to pressure.

Second—Recent cases, attributable to exposure to excess of light.

Lastly—It should never be used in cases of acute inflammation of any of the textures of the eye.

And in all cases admitting a hope of recovery, the examination should not be uninterruptedly continued for more than two minutes, those cases only excepted in which there is considerable opacity either of the lens, capsule or cornea, or great insensibility to light.

A prudent regard for his own eyes also will prevent the observer prolonging in any case the examination much beyond one minute.

The consideration of cases in which the ophthalmoscope may be used will, with some colored illustrations, be reserved for another paper.†

* I have quoted from myself, Boston Med. & Surg. Journal, Dec. 1854.

† Since the above pages were prepared, I learn from Dr. W. C. Wallace of New York, that for more than twenty years past he has been in the habit of examining eyes by artificial light, by the aid of an apparatus similar to a magic lantern, and that although he has used it chiefly for the examination of the lens and posterior chamber of the aqueous humor, he is also able to see imperfectly the retina itself.

In an article by Dr. Wallace upon the physiology and pathology of the

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ART. II.—*An Essay on Syphilis.* Read before the Society of Alumni of the Medical College of Virginia, and published by request. By THOMAS POLLARD, M. D. Richmond, Va.

THE subject I propose to introduce to your attention at this time is syphilis. Though the theme is hackneyed, the subject is by no means exhausted, and is still, I conceive, not well understood in several respects; and one object I have in presenting it to you is the hope that I may induce you, in your opportunities of observation, to give the disease your close, scrutinizing study.

It is certain that there are many disputed points in syphilis. I do not hope to settle them, for my opportunities of observation do not enable me to speak *ex cathedra.* I only design to collect and compare the opinions of the best authorities on the subject, and bring before you the latest observations and facts bearing on it.

Of course, in a subject so extensive, I cannot expect and do not propose to touch upon every portion of it, or to give you a "treatise on syphilis," but to confine myself principally to the disputed points in the malady.

Even the *origin* of syphilis has been much disputed and keenly argued, but its history, I think, proves that it was either introduced in Europe or originated there about the close of the 15th century. Certain it is that about this time it assumed suddenly a very severe form, and probably for the first time attracted public attention. Ricord, how-

nervous system of the eye, published in the Boston Medical and Surgical Journal April 12, 1848, will be found (p. 209) the following remarkable passage:

"Long before the catoptric test was heard of, I was in the habit of examining patients with a magic lantern, either in a dark room or under an umbrella, with a curtain round the margin. I was enabled to recognize cases of cataracta pigmentosa, closure of the pupil, &c. which had previously been treated as amaurotic affections. With the magic lantern unaffected by adventitious light, not only may any opacity of the humors be detected, but *opaque spots may sometimes be seen on the retina.*"

Dr. Wallace also states to me, that in his long and frequent use of the intense concentrated light from this instrument, he has never seen any injurious effect.

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ever, to whom I shall have occasion frequently to allude, and who has made this subject the study of his life, and who is perhaps the best living authority upon everything connected with syphilis, expresses the opinion that it is of very ancient origin.

On the occasion alluded to, its features were much more strongly marked, and much more severe than we see it in modern times, if we except the form which prevailed in the English army in the peninsula war, and in the French army during Napoleon's German campaigns.

Unity of the Syphilitic Virus.

On this subject modern authors are divided. M. Clerc, the celebrated Parisian syphilographer, who at his dispensary treats annually from four to five thousand cases, and M. Bassereau, who has written a very able work on syphilitic diseases of the skin, believe that there exists two distinct varieties of syphilitic virus—the one attended with indurated or infecting chancre—the other with simple nonindurated noninfecting chancre—each transmitting itself according to its variety.

In support of this opinion, it is urged that in syphilitic hospitals it is common to see patients cured of nonindurated chancres by local applications alone, without the supervention of secondary symptoms. These same patients expose themselves again, and in the space of a few weeks they are affected with indurated chancre, followed with constitutional symptoms. Benjamin Bell relates a case where the same woman communicated to many persons at the same time the identical form of chancre, and Clerc relates many cases,* where he was enabled to trace the source of the infection, in which chancre always propagated its own variety. The simple nonindurated variety Clerc believes to occur in persons who have previously suffered from constitutional syph-

* See Memoir of M. Clerc on Syphilitic Chancroid. Translated by Dr. Geo. Starke, in No. 27, Va. Med. and Surg. Journal.

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ilis, and thinks it is the analogue of varioloid. This latter doctrine involves the proposition of one attack of constitutional syphilis exempting the system from future attacks of the same kind, and of which we shall come shortly to speak.

On the other side of this question stands Hunter and Ricord, though Ricord expresses himself in some doubt when he says this subject is sufficiently obscure, and not well settled. He, however, lays down the doctrine of unity of the syphilitic virus, and says that a chancre is infecting or indurated according to the predisposition of the individual, and that this predisposition determines the variety. He relates (in the last edition of his Letters on Syphilis, 1856) a case of an "interne" in the "Hopital du Midi," on whom an indurated chancre was produced by the pus of nonindurated chancre by inoculation. At the same time he remarks "that the difference which exists between the infecting indurated chancre and that which remains local, is so great, and the fact that the indurated chancre is so frequently, even so regularly derived from the indurated chancre, as I have so often remarked to my pupils, that the opinion again put forth by my distinguished pupil M. Bassereau is very defensible."

Chancre.

The description of chancre by Hunter is almost universally received at the present day, and to him is due the honor if not of its discovery, of accurately describing indurated chancre. Jean de Vigo is said first to have observed it. To Ricord belongs the honor of pointing out the connection between indurated chancre and constitutional syphilis. That this was not observed by Hunter is considered by Ricord as a grave and inexplicable omission in a man of his sagacity and divination.

In describing chancre, Hunter says it first begins with an itching of the part, this itching being gradually changed to pain. The surface of the prepuce in some cases ulcerates—

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in others, a small pimple or abscess appears, which forms an ulcer. "A thickening of the part comes on, which at first is very circumscribed, not diffusing itself gradually and imperceptibly into the surrounding parts, but terminating rather abruptly. Its base is hard, and the edges a little prominent."

Ricord says that a chancre has no premonitory symptoms, the sensation of itching not being constant. The induration which is generally clearly circumscribed, and as Bell says, is not unlike the half of a pea placed beneath the ulceration, he contends never precedes the chancre, as Mr. Babington has said. The solution of this question and the time of the appearance of the induration is important, for according to his doctrine, the moment the induration takes place, the disease is not alone local. It is attended with much difficulty, but from his observation he lays down the rule, "that it never commences before the *third* day; in all cases, it is in progress from the 7th to the 14th day; and that if a chancre exists for more than the third week without indurating, it will never indurate."

We may be deceived by this test of induration. Common inflammation often masks the specific induration so as to deceive us, and induce us to believe that inflammatory and phagedenic chancres may be followed by constitutional infection like indurated chancres. But in these cases induration precedes and has been destroyed, and masked by phagedena and inflammation, and there is no infection where there has been no induration. And again—various applications, such as acetate of lead, corrosive sublimate, nitric acid, nitrate of silver, &c. sometimes produce artificial indurations, which may counterfeit specific indurations to perfection; but in these cases symptomatic swelling of the glands is wanting, as also constitutional symptoms at a later period. These artificial indurations, too, are of much shorter duration than specific induration, unless they are kept up by continuance of the causes which produced them.

On the subject of the locality of chancre, it may be

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further remarked that chancre is not confined, as at one time supposed, to nonsecreting surfaces. Ricord says they are found not only on all parts of the skin, but on the mucous membrane of the lower part of the rectum, throughout the vagina, on the neck of the uterus. Even in the cavity of this organ, in the male and female urethras at different depths, in the cavity of the mouth, on the lips, tongue, posterior and superior walls of the pharynx, and on the palpebral and ocular mucous membranes.

Simple and phagedenic chancres do not produce constitutional symptoms.

Bubo.

The modern doctrine on this subject can be stated in a few words.

1. A bubo, preceded by a nonindurated chancre, almost always suppurates.

2. A bubo following an indurated chancre, passes in its turn into a state of induration.

The first variety of bubo is what Ricord calls the bubo of absorption. Inoculation from this variety produces simple nonindurated chancre.

The second variety rarely suppurates, and when it does, the suppuration is the consequence of common causes, and is situated outside the indurated ganglion. Experiment proves that the pus derived from it is not inoculable.

In the first variety the bubo is frequently confined to one ganglion. In the other, several are swollen, and the development of the latter is more rapid than the first.

Constitutional developments are not dependent in any manner on the suppuration or nonsuppuration of the varieties of bubo.

There are benignant buboes, and secondary or constitutional buboes, which need not occupy our attention here. The question whether syphilitic bubo follows impure contact, (the "bubo d'emblée" as the French call it,) without

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previous chancre, is an interesting one. Hunter seems inclined to believe it sometimes does, though he says, "if the parts are explored with much care, or if the patients are minutely interrogated, it is probable that one would discover that a small chancre is the cause of the infection."

Bell, Lagneau, Diday, Gibert, Erasmus Wilson,* (syphilis by Erasmus Wilson, 1852,) take the affirmative of this question—Ricord the negative. His reasons for his position are that these buboes rarely appear before the second week after the suspected act; generally later. That experiment proves that the moment virulent pus is placed in contact with the tissues beneath the epidermis or epithelium, that moment a local action of necessity commences and leads to ulceration; and that if pus be placed on a sound surface, capable of remaining sound, and allowed to stay there any length of time, buboes are never produced, which afterwards present the character of chancre, as do those which are really the consequences of this antecedent. When we recollect how easily we deceive ourselves or allow ourselves to be deceived by patients, we can explain those cases which are regarded as exceptions to this law.

Concealed chancre is frequently a cause of what is considered nonconsecutive bubo, or "bubo d'emblée," this chancre being concealed in the urethra more frequently—sometimes in the vagina and rectum.

These buboes never furnish inoculable pus, as Ricord has demonstrated, and this fact seems conclusive in support of his position.

Constitutional Syphilis.

This branch of the subject is so extensive, that I can only be expected to touch on some few prominent points of it. The period at which constitutional syphilis manifests itself, is usually from six weeks to six months. Occasionally, it exhibits itself in two weeks, and then again it may be years

* Author of the work on skin diseases.

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before we see any sign of it, this late appearance being generally due to the giving way first of the health from other causes.

Cases are recorded, where it has been twelve years, and I think in some instances it has been known to develop itself at a longer interval. Ricord lays it down as a law, that where no remedies have been used, "the appearance of secondary symptoms is never postponed beyond the *sixth* month." These secondary symptoms attack the mucous membranes and the skin, and develop themselves in ulcerations, eruptions, warty growths, mucous papules, mucous pustules, the "plaque muqeuse," and the "pustule muquese" of the French, while the tertiary symptoms fix upon the periosteum and bones.

Ricord, in speaking of the causes of constitutional syphilis, and laying down the proposition that the pus of chancre alone was inoculable, and susceptible of reproducing chancre, has found it necessary to lay down the following rule: That essential gonorrhœa, not symptomatic of chancre, is not inoculable—that is, that it can never give place to chancre and its consequences.

Hunter believed the poison of gonorrhœa was similar to that of syphilis. Ricord has fully demonstrated their difference, by both experiments in inoculating the gonorrhœal matter, and from observation. Still, men of learning and talents adhere to this as I conceive exploded doctrine. M. Vidal, in his work on venereal diseases, (Paris 1853,) admits the existence of nonvirulent gonorrhœa, but contends that in most cases gonorrhœa and syphilis are due to the same cause, viz: a specific virus. Erasmus Wilson, in his work already cited, contends also for their identity, and among other cases, details what he believes to have been an instance of the occurrence of secondary constitutional syphilis from gonorrhœa. The patient was a medical man, who he says felt assured that he could not be suffering from chancre of the urethra. These apparently anomalous results Ricord attributes to concealed chancre, and when by inoculation of

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muco-pus, or pus from a mucous surface, which he has not explored, he has attained positive results, he is forced to infer the existence of concealed chancre, which diagnosis he has always verified by means of the speculum in woman, and in man, by separating the lips of the meatus, when it is often easy to see the specific ulcer at a greater or less depth in the urethra. Sometimes too deep to be seen, the chancre only develops itself externally by the production of induration, or by perforating the canal and involving the external parts. He cites two cases where pathological anatomy has added a final argument. He has shown to the "Academy of Medicine" two urethras, taken from patients who died with symptoms of gonorrhœa, and in whom he diagnosed urethral chancres. Autopsy showed that both had chancres at different depths, and one of them even in the bladder.

A very interesting point in the consideration of constitutional symptoms is the contagion of secondary accidents; and here again we find considerable diversity of opinion among different observers. Erasmus Wilson introduces cases to establish the doctrine that secondary accidents are transmitted from a man to a woman, and conversely, by cohabitation, through the contaminations of the secretions, and that in some cases, where the infecting individual is to all appearances cured of his secondary symptoms; and even relates a case where he believes one party, who has suffered from primary symptoms without subsequent development of secondary symptoms, has communicated secondary symptoms by cohabiting with another.

Clerc, Bell, Velpeau and others believe in the contagion of secondary accidents. Hunter believed the doctrine only in part, as does Ricord. Hunter, while disbelieving in the general contagion of secondary accidents, maintained their hereditary transmission, and the communication from the nurse to the child, and from the child to the nurse. He records an instance where three wet nurses were in their turn contaminated by the same child, two of whom gave the disease to their own offspring. Lawrence records like cases,

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as also Dr. Colles; and in Ranking's Retrospect, vol. 4, very strong examples of the same character may be found.

Ricord lays down the proposition that constitutional syphilis, secondary or tertiary, cannot be inoculated, and therefore cannot be contagious. He admits the transmission of secondary accidents from the father or mother to the fœtus, and from the fœtus, when contaminated, to the mother, and from an infected nurse to an infant, by means of the milk. This last is denied by Acton, a celebrated English syphilographer. But he does not admit that an infected child can give the disease to the nurse—a point much mooted at the present time. In support of this opinion, he cites the authority of some half dozen medical men, who are the heads of foundling and venereal hospitals in France, who say, after much experience, that they have never seen secondary accidents thus transmitted. He relates the history of an infant, watched by himself and Dr. Chailly, affected with secondary symptoms, particularly with deep ulcerations of the lips, and afterwards of the soft palate, who sucked for 18 months the same nurse, without communicating to her any symptoms. Also, an instance recorded by M. Seux, where an infected infant nursed for ten months three different nurses, without communicating to them any disease. But one positive fact outweighs any number of negative ones. A case is related to me by a medical friend, recently returned from Paris, to this effect. Clerc exhibited to Ricord a nurse with secondary sores on the breast, and asked his opinion of it. Ricord admitted to him its nature. The child was then produced, which had been nursing the woman, and was found to be suffering from "plaque muqeuse," which Clerc contended, with every appearance of truth, had communicated the disease to the woman. The parents of the child affirmed positively that the nurse had not been out of their sight or observation for six months.

If Ricord's experiments of the noninoculability of secondary accidents were certainly true, it would seem to settle the question in his favor. But Velpeau contends that he has

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succeeded in inoculating secondary accidents. This question is of much importance in a medico-legal point of view, as action has been more than once brought by women who contended they were contaminated by children they had nursed.

Exemption from Second Attacks of Constitutional Syphilis.

The doctrine, that syphilis is a contagious disease following the law of the other contagious diseases (of exemption to individuals from second attacks,) is of modern origin, and belongs to the French school. It is a question of deep interest, and like other innovations upon established opinions, is calculated to startle us and excite our incredulity. Still, I believe an attentive examination of the facts upon which it rests, derived from an observation in hospitals and dispensaries, where many thousands of patients are annually treated, and the test of inoculation to which it has been subjected, if it does not convince us of its truth, will induce us to treat the opinion with respect, and inspire a wish to investigate it with all the means in our power.

This important doctrine was originated by Ricord, and has been ably sustained—I am almost tempted to say demonstrated—by him. He lays it down in these words: "As a general law, a patient who has once had an indurated chancre, does not have another."

He reasons on the subject thus:

When there has been an indurated chancre, there is *necessarily* constitutional syphilis.

With induration, the syphilitic *disposition*, as Hunter calls it, is acquired.

The syphilitic *temperament*, as I designate it, is established.

There is then a *diathesis*, a special disposition, in virtue of which, ulterior manifestations are produced.

Disposition, temperament, diathesis, are not doubled, are not multiplied, more than the analogous disposition in the vaccine disease.

Indurated chancre is to syphilis what the true small-pox

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pustule is to variola, or the vaccine pustule to the vaccine disease.

Nonindurated chancre is the pseudo pustule—it is the false vaccination—it is the chancroid, if you wish, of M. Clerc.

Observation seems to confirm this plausible theory, based on inductive reasoning. MM. Clerc, Puche, Diday, as well as Ricord, with other observers, bear witness to its truth, and agree in saying that indurated chancre, with a second contamination, never follows constitutional syphilis. Patients with indurated chancres and constitutional symptoms have accidentally inoculated themselves with pus from chancres, and the chancres are found nonindurated. Such cases particularly are detailed by Clerc. Inoculations testing this point have recently been performed in Paris; and I am informed by the medical gentleman above alluded to that there is, or was a short time since, a German student in Paris, who had suffered from constitutional syphilis, and had inoculated himself many hundreds of times with pus from indurated chancre, without producing any effect but simple noncontaminating chancre.

Period of Incubation of Syphilitic Virus.

On this point, Hunter says that the distance of time between the application of syphilitic virus and its effects is uncertain, and that it is longer in appearing than gonorrhœa. He has known cases where it appeared in 24 hours after the application of the virus, and quotes Swediaur as saying that he has known chancre to occur in 12 hours—and in others, where it has been seven weeks, and even two months, as in the case of an army officer, who had chancre after this time had elapsed from his connection with any woman. I have seen a case in a medical man, where six weeks intervened between the connection and the production of chancre. Ricord says he has demonstrated experimentally that the formation of chancre is not preceded by a period of incubation, and that the moment the specific virus is placed in contact

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with the tissues in such a manner that infection is possible, a process is set up which goes on incessantly, though with various degrees of regularity and rapidity in different cases, until it produces a perfect ulcer. The late appearance of some chancres is explained by the fact that their evolution is slower in some individuals and on some tissues than in others, and by various other circumstances. Thus, many patients do not discover the existence of a chancre until it has already acquired a considerable size, or become painful. He further remarks, that virulent pus may be separated for a long time from the ulcer which produced it, without losing its contagious property, and that it may remain on the tissues without effect until they open a door to it; and thus we may account for those chancres which Hunter saw seven weeks after connection. At any rate, he continues, it is allowable to say, contrary to what Hunter advances, that a chancre appears sooner after the application of virus than gonorrhœa does after the various causes to which it may be due. It is certain the virus remains a long time dormant; and why not call this incubation. Gonorrhœa, it seems to be admitted, has a period of incubation, for the pus can scarcely remain simply dormant in the urethra over which the urine is constantly passing; and why not concede incubation to syphilis? Besides, the theory of incubation would carry out more perfectly the analogy claimed for syphilis and chancre to variola and varioloid.

Treatment.

The best authorities agree that mercury and iodide of potassa with caustics are the sheet anchors in the treatment of syphilis—mercury for the primary and secondary symptoms, and iodide of potassa for the tertiary.

Should mercury always be used for secondary symptoms? We think not. If it has been freely given for primary symptoms, or if the patient has become cachectic, we should desist from its further use. It is in these cases that nitro-

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muriatic acid comes well into play. And under these circumstances also, we may use iodide of potassa; and if much emaciation occurs, iron and cod-liver oil. When this cachexia is removed, then, if necessary, mercury may again be resorted to.

Ricord has laid down the following rules for the use of mercury in this disease:

1. Give mercury internally when the state of the digestive organs admits.

2. Apply it to the skin in contrary cases.

3. When the mucous passages are irritated too soon, and the skin is equally irritable, alternate the two methods.

4. Some patients, who cannot be reached either by the skin or digestive passages, are favorably affected by inhalation of mercurial vapors in many cases.

5. It is rarely necessary to wait longer than a week to perceive the effects of mercury either as a morbific or curative agent; and so long as no favorable change is produced, the daily dose should be increased every week, unless contraindicated by some supervening symptoms.

6. As soon as a favorable change is produced, we should stop at the quantity which produced it, and only increase it when the disease arrives at a status quo.

If mercury produce any unpleasant symptoms, the mode of using it should be modified, or it should be entirely suspended.

8. When the mercurial symptoms have subsided, and the syphilitic symptoms remain, mercury should be resumed with such modifications as the peculiar nature of the former require.

9. The same inconveniences do not always reappear on resuming the remedy after suspending it or simply modifying it.

It should not be used in phagedenic chancre.

Clerc calls the simple nonindurated chancre, chancroid, and believes it should not be treated with mercury; and Ricord, when he thinks the chancre nonindurated, dispenses

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with mercury. I am afraid that we in this country are disposed to use mercury too freely and indiscriminately, regardless of the character of the chancre we have to treat.

Of the particular form of mercury, the iodide is probably the best to be used. When it is required to produce a moderate effect in persons somewhat delicate, without producing much irritation or salivation, it is certainly to be preferred.

With regard to the treatment of chancre, mercury should be used until the *induration* is removed. In the local treatment, Hunter advises nitrate of silver, potassa fusa, and excision of the chancre. Ricord prefers Vienna paste, directing not only the destruction of the chancre, but the surrounding tissues for a short distance. He also extols nitric acid and a mixture of concentrated sulphuric acid and powdered charcoal. Nitric acid, he says, is more painful than Vienna paste, and requires more frequent repetition. Other authorities, among them Erichsen, (Surgery, 1854,) says that nitric acid rarely requires but one application, and that it is not more painful than nitrate of silver.

Ricord on this subject, lays down the following propositions:

1. A chancre is at first a local affection, and continues so until *induration* supervenes.

2. As soon as a chancre becomes indurated, (and this rarely happens before the fifth or sixth day,) the disease ceases to be simply a local affection, *induration being a certain proof that the system is contaminated.*

3. There are two things to be considered in the cure of a chancre—the cicatrization of the ulcer, and the state of the tissues beneath it. A cure is really effected only when the induration is removed.

4. We are to regard mercury as the most efficacious special treatment for indurated chancres that we possess, induration being an indication that constitutional syphilis already exists, over which, as Hunter judiciously remarks, mercury has frequently more control than over the primary ulcers, especially when the latter is not attended with induration

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5. The early administration of mercury in cases of indurated chancre may anticipate the development of constitutional symptoms.

6. The duration of mercurial treatment is not to be measured by the cicatrization of the chancre, *but by the entire disappearance of the induration*; and even this is not always sufficient.

ART. III.—*On Tracheotomy in the Last Stage of Croup.* By ARMAND TROUSSEAU, Professor of Clinical Medicine at Hôtel-Dieu of Paris, etc. etc. etc. Translated from the Archives Générales de Médecine, with some Additions. By GEORGE A. OTIS, M. D.

MEDICAL men who have confidence in their art, and desire to extend its limits, very often become, especially when they are young, the enthusiastic advocates of some particular operation or therapeutical method. Then age comes, which calms even the best of passions, and experience, which age does not always bring with it. Distrust and skepticism arise, or a blind self-confidence; the physician acquires the habit of believing in his own infallibility, or in the inanity of the efforts of others; he makes no farther progress; he pauses, and presently retrogrades.

But when, in the course of a long medical career, one has contributed to the propagation of a useful idea; when each year brings numerous facts to add to others accumulated already, and the success obtained in the method he defends steadily augments, he finally becomes a zealous apostle, and devotes himself till death to the cause he has espoused.

That is my position in respect to *tracheotomy in the last stages of croup!* Brought into honor by my master, Dr. BRETONNEAU, and successfully practiced by him for the first time, this operation was carried by me to Paris, when it could boast of only one cure; and it will be readily understood what opposition I sustained, so young as I was then, without experience, without authority, not even professing

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to practice surgery; and what difficulties I surmounted in my effort to popularize and introduce into the ordinary domain of therapeutics a strange and unheralded method.

It is singular, but no less true, that the most formidable obstacles were put in my path by the surgeons. And even now, if I except Dr. Guersent, (secundus,) the majority of surgeons dislike this operation; not, surely, from any inability to do it well, but because the proportion of recoveries after their operations does not satisfy them.

In some European countries, and especially in England, tracheotomy in croup is such an unusual operation, that in all Great Britain, indeed, it is less frequently performed than in the city of Paris alone.

I have often enquired the cause of this repugnance to the operation, and I find it in an opinion which is completely false, namely, that tracheotomy in the last stages of croup is rarely successful. Unhappily, as the question involves a surgical operation, reliance is placed on the opinion of the great surgeons, and when these eminent men refer to their own operations and invoke their own experience, they have so many reverses to relate, that they discourage all who hear or read their views.

And men like Dr. Bretonneau and myself, who are simple physicians, who have practiced only medicine all our lives, when we are placed in opposition to the most eminent practitioners of surgery, what authority can we have? Our surgical incapacity is well known, we don't seek to disguise it in the least, and accordingly distrust is felt in respect to our partiality for an operation, which we are supposed to advocate through obstinacy and prejudice.

For my part, I have determined not to be discouraged, and to preach tracheotomy with more and more confidence as its success increases, and even if its results were only what they were ten years ago, I would still proclaim its necessity, I would still declare it *a duty*, as strict a duty as it is to tie the carotid artery when wounded, although death follows this operation, assuredly as often as recovery.

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I wish to give, in brief words, the results of my operations for tracheotomy in 1854. I did the operation *nine times*. Two of the children died; *seven* recovered, and are now alive and well.

As I rarely perform tracheotomy except in the practice of my brethren, in as much as I see few patients as their physician in ordinary, I will enumerate the physicians who have done me the honor to call me in consultation, and the names of the families where I had the happiness of being instrumental in obtaining cures.

1. M. Thibaut, silk merchant, rue Vivienne, 14; physician, Dr. Dechamps.—*Recovery*.
2. M. Jacome, lithographer, rue Meslay, 63; Dr. Poulletier.—*Recovery*.
3. M. * * *, Place de l'Estrapade, 35; Dr. Dewulf-Pontonnier.—*Death*.
4. M. Powret, apothecary, rue du Rambuteau, 100; Dr. Lemarchand.—*Recovery*.
5. M. Riviere, huckster, rue aux Fers, 7; Dr. Lemarchand.—*Recovery*.
6. M. Proust, merchant, rue Culture—St. Catharine, 13 *bis*; Dr. Charpentier.—*Recovery*.
7. M. Francis, messenger, cloître Ste. Opportune; Dr. Lemarchand.—*Death*.
8. M. Samson, clothing store, rue de Rivoli, 40; physicians, Drs. Brossard, Latracharde and Aussandon.—*Recovery*.
9. M. Millot, dry goods merchant, rue Vivienne, 2 *bis*; physician, Dr. Horteloup.—*Recovery*.

Here are seven cures in nine operations, the best results I have ever obtained.

Most assuredly, the proportion of cures is not always so great; but when I examine the list of operations I have performed in the last four years, I find *fourteen recoveries* in twenty-four operations: *more than half*.

At the hospital for sick children, *l'Hopital des Enfants Malades*, the proportion of cures, in the last five years, has been *nearly one-fourth*.

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Here are the official statistics of that institution for the last five years:

1850—20	operations,	6	cures,	nearly a third.
1851—31	"	12	"	over a third.
1852—59	"	11	"	a little less than a fifth.
1853—61	"	7	"	only a ninth.
1854—44	"	11	"	a fourth.
Total, 216	operations,	47	cures,	nearly one fourth.

This result is a matter for congratulation, when we consider the social condition of the children brought to this hospital, the deplorable treatment to which many of them are subjected by midwives, quacks, and old women before admission, and, most particularly, the disadvantageous condition of the hospital itself, where the children are exposed, after the operation, to varied and serious contagious influences, so that, oftentimes, when everything has been going on well after an operation for tracheotomy, the little patient has contracted a disastrous complication in the shape of variola, scarlatina, rubeola or hooping cough.

There is no doubt in my mind that, in private practice, cure in half the cases should be the rule, provided the operation is done under circumstances in which a cure is possible.

This restriction is important, for, if diphtheritic infection has profoundly invaded the economy, if the skin, and especially the nasal fossæ, are occupied by the specific phlegmasia, if the frequency of the pulse, the delirium and prostration indicate a general intoxication, so to speak, of the organism, and if, in a word, *the peril arises from the general state of the system and not from the local lesion of the larynx and trachea, the operation ought not to be undertaken,* for it is invariably followed by death; whereas, when the local lesion constitutes the principal danger of the disease, whatever degree of asphyxia exists, even if the child has but a few instants to live, tracheotomy succeeds almost as well as if performed three or four hours earlier.

It will be understood that the skill of persons accustomed

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to performing surgical operations will always be superior to mine. Whatever habitude I have in an operation I have done more than two hundred times, I still perceive that my hands and eyes grow old fast, and I always feel glad now, when a surgeon or young physician will take my instruments and let me assist him. On this point there is no difficulty. Any one can do the operation as well or better than I, provided he will give himself the trouble of learning how to do it.

In fact, as in the simplest manœuvres of surgery, it is there is a right and wrong method; and though tracheotomy is as simple as phlebotomy, yet one does not confide the veins at the bend of the elbow except to one who knows how to find them, how to open them, and especially how to avoid the delicate parts about them. So it is in tracheotomy. You must know how to go about it, and you must have suitable instruments.

What is to be done is to open the trachea, and the trachea alone. Many practitioners wish to open the crico-thyroid space, to cut the cricoid cartilage, and the first two rings of the trachea. A moment's reflection is sufficient to comprehend that, in so doing, the larynx itself is necessarily entered, and that if, as often happens, a canula is kept in the wound several weeks, a partial necrosis of the cricoid and thyroid cartilages will be likely to ensue, whence grave ulterior accidents, sometimes involving an entire loss of the voice, are liable to result.

We must then enter the trachea and the trachea only.

The procedures vary.

The most brilliant is that described by Heister. (*Instit. Chirurg.* P. ii, p. 722.) It consists in penetrating at once into the trachea with a canula armed with a trochar, which is withdrawn, leaving the canula in place. Van Swieten prefers a slower method: *Tentavi aliquoties in cadavere et in vivis animalibus hanc methodam, sed videbatur mihi admodum difficilis, et non carere periculo, ne quandoque valida vi adactum instrumentum deviaret. Unde crederem, priorem methodum, licet*

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magis operosam, præferendum esse. The method devised by my lamented friend, Auguste Bérard, is analogous to that indicated rather than advised by Heister. He made sure of the position of the trachea; tried to steady it between the index and middle finger, entered the point of his bistoury at the level of the cricoid cartilage, pushed it inwards until the escape of air indicated that the trachea had been reached, then prolonged his incision so as to divide three or four rings, introduced the dilator, and placed the canula *in situ.* It was done in an instant. But terrible hemorrhages taught Bérard that the quickest way was not always the best. In later years, he gave up his expeditious operation, for the more vulgar and surer method.

Dr. Paul Guersent also adopted what I call the expeditious method. He soon recognized its danger. Now-a-days, though he operates much better and much quicker than those of us who are not surgeons, he proceeds slowly enough to avoid those dangers that all eminent surgical practitioners have observed and proclaimed.

I dwell the more on the necessity of incising the tissues one by one, of drawing aside the vessels with a blunt hook, of completely laying bare the trachea before opening it, because tracheotomy being destined to become a very common operation, will be practiced oftener by physicians than by surgeons, and woe to the patient, if a man unfamiliar with surgery does not make it a duty to be *too slow* in his operation. I never saw slowness the cause of an accident, yet over and over again I have witnessed the difficulties and dangers of rapid tracheotomy, even in the hands of consummate operators.

I said a while ago that it was necessary to have suitable instruments. These instruments are a straight or convex bistoury, a probe-pointed bistoury, a dilator, two blunt hooks or bent wires or pins to answer for them, and a *double canula*, the diameter of which should be larger, and cannot be too large provided the instrument readily enters the trachea.

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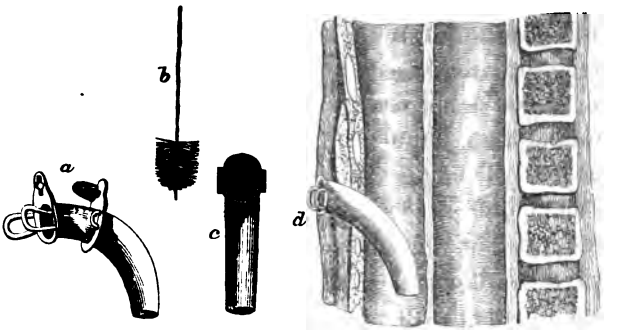

FIGURE I.

Canulæ of different forms—half the natural size.—*d*, a single canula in profile with a vertical section of the neck; *c*, single canula, the antero-posterior exceeding the transverse diameter; *a*, the double canula of Borgellat; *b*, brush for cleaning inner tube. After Vidal.

The double canula is absolutely necessary, and in truth, when we see how Van Swieten (Comment. II. p. 528)* insists on the utility of a double tube, quoting in its behalf the authority of the English author Martine,† we wonder how this precept has been forgotten, notwithstanding the recommendation of Dr. Bretonneau, who, from the commencement, used a double canula. For a long time, I myself neglected to make use of it.

Of the instruments I have mentioned, those which are indispensable are an ordinary knife, blunt hooks of some sort, a dilator, and double canula. On one occasion I have

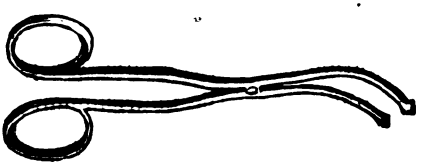

FIGURE II.
Trousseau's Dilator.

lost my patient during the operation. It was a little child

* Volume vii, p. 121, *et seq.* of the Edinburgh translation of 1776, the most common version of Van Swieten in this country.—*Translator.*

† Martine seems to have investigated the subject of tracheotomy with much care, and to have had considerable experience in the operation. His views are printed in the *Abridgment of the Philosophical Transactions*, vol. vii, p. 496, etc. and will be alluded to hereafter.—*Translator.*

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attended by my excellent friend Dr. Barth. I reached the place of consultation without knowing what was the matter, and found the child moribund. Dr. Barth had a bistoury and canula. I could not draw aside the vessels as I desired, I had no dilator; I fumbled some time before I could get into the trachea. The blood entered the air passages, and suffocated the child—a casualty which surely would not have occurred if I had been provided with a dilator, by means of which I could immediately have placed the canula.

When the trachea is once opened, little matter how, in truth, whether the operation has been done bunglingly or skillfully, rapidly or slowly, things become equal, provided hemorrhages have been avoided, for loss of blood has a most unfavorable influence as regards the results of the operation.

There remains the question of treatment. I should do wrong to speak only of the after treatment; a great deal depends on the treatment antecedent to the operation. Let me say at once, for it is the exact truth, that the majority of physicians are now-a-days happily convinced that general medication and revulsives are perfectly useless; and, consequently, the little patients are not exhausted by blood letting or irritated by epispastics. I am convinced, for my part, that my good fortune in the operations I have practiced of late years, is due rather to the improvement in the treatment instituted by my professional brethren, than to the method I have followed.

When the operation is over, the first thing that calls for the physician's solicitude, is the nourishment; nourishment, that all-important remedy in the majority of acute diseases, and especially in the diseases of childhood.

Abstinence, laid down by Broussais as essential in the treatment of phlegmasiæ, and still advised by a multitude of physicians, who cannot shake off the old man, and who cling to the prejudices of their early medical training—abstinence, I say, is assuredly one of the most deleterious complications of disease, the most likely of any to promote a general infection of the organism, to open the way for the absorption

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of external miasmata and of vitiated excretions elaborated in the diseased body, and the most antagonistic to that faculty of resistance which is the grand *primum mobile* of convalescence and ultimate recovery.

I do not mean to say that the little patients must be stuffed with food; but simply that the appetite is to be satisfied, if they have any, and that they must be induced to eat if they have a repugnance for food. Oftentimes, assuming a severe expression, I have constrained children to take nourishment, and have thus prepared the way for recoveries I should otherwise have regarded as impossible. Milk, cream, eggs, chocolate and broths are the articles on which I chiefly insist.

What I have said is sufficient to indicate that I forbid, in the most formal manner, a continuation of the means which may have been thought useful before the operation, such as calomel, alum, purgatives and emetics in general, which are totally incompatible with the alimentation which I regard as so essential.

It is scarcely necessary to say that blistering is pernicious; the more so because it establishes new surfaces on which diphtheritis may be developed, and whence absorption may take place, and that general infection which must be avoided at all hazards. Inasmuch as we often find blisters already applied, I may mention that the denuded surface should be dressed at once with rhatany ointment, or with Goulard's cerate; or, if the skin so unfortunately and uselessly denuded is already covered with diphtheritic concretions, it should be vigorously penciled with nitrate of silver.

I now come to some details respecting the dressing of the wound, details to which I may seem to attach an exaggerated importance; but, the older I grow, the firmer becomes my conviction that, in therapeutics, more depends on minor matters than is commonly acknowledged.

A bit of court-plaster or india-rubber cloth should be interposed between the canula and the skin to protect the wound from irritation.

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The relatives should be instructed to remove the inner tube every two or three hours, to clean and replace it.

The throat of the child should be enveloped in a scarf of netted worsted or by a large piece of muslin, so that the inspired air may be warmed and impregnated with the vapor furnished by the expiration. This precept is of capital importance. By acting in obedience to it, we avoid dryness of the cavity of the canula and of the trachea, irritation of the mucous membrane, and the formation of those coriaceous crusts, analogous to those formed in the nasal passages of persons suffering from coryza, crusts which sometimes separate in complete tubes, or segments of tubes, and produce most terrible paroxysms of dyspnœa, and death by obstruction of the canula.

Before Dr. Paul Guersent and I adopted this precaution, we lost a great number of our patients with catarrhal pneumonia. This affection now occurs far less frequently. It is probable that a supply of warm moist air to the bronchi is a condition most favorable to the success of the operation.

There is yet another practice without which recovery is rare; I refer to cauterization of the wound. During the first four days, the surfaces of the incision should be penciled daily with a vigorous hand. Thus we prevent a redoubtable complication, diphtheritic infection of the wound, otherwise, the lips of the incision may become covered by thick and fetid false membranes; the specific inflammation may extend to the neighboring cellular tissue, producing a phlegmonous erysipelas of a bad type, local gangrene, or, at the very least, a violent symptomatic fever which is rarely subdued. After the fifth day, the surface of the wound is modified, and the accidents I have described are no longer to be apprehended.

There is a part of the treatment still remaining, on which I wish to say a word; it relates to the removal of the canula and the closure of the wound.

It may be laid down as a principle that the sooner the tube is removed, the better. It can hardly ever be removed

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safely until six days have elapsed; it is rarely necessary to leave it over ten days. Yet there are cases in which the functions of the larynx are not restored until the fifteenth, twentieth, or even forty-fourth day, as I had occasion to observe in a little girl, who finally recovered.

At the close of the first week, the canula should be gently removed, every pains being taken to avoid frightening the child. The poor little ones are so accustomed to breathe through the artificial orifice, that when this is closed to facilitate the passage of air through the glottis, they are terrified, and cry and struggle, thus accelerating the respiratory movements. The larynx is still somewhat obstructed, either by slightly adherent false membranes, by mucus, or by tumefaction of the mucous membrane; moreover the laryngeal muscles have lost the faculty of harmonious action. Great dyspnœa arises; this will often subside if the child is quieted, a task which the mother will accomplish better than the physician. The wound should be united by strips of isinglass plaster. If the cough, respiration and voice indicate that the orifice of the larynx is patulous, you let things alone. But if the air does not pass freely, you remove the strips, and simply lay upon the wound a cibriform compress spread with cerate, and defer closing the incision until the next day. If the air does not pass the glottis at all, you replace the canula, and renew the attempt a few days subsequently. As soon as the respiration is executed naturally when the wound is occluded, dressings should be applied twice a day, and usually the opening into the trachea will be closed in five days. There remains only the external wound, which is to be treated as a simple sore.

There is one rather serious accident to which I long ago called attention, which Dr. Archambault has recently investigated with care. I refer to the difficulty of deglutition which sometimes supervenes. The difficulty consists mainly in the passage of drinks through the glottis, which provoke a convulsive cough whenever the child attempts to swallow. Besides the serious results which may arise from the contact

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of liquid food with the mucous membrane of the bronchial tubes, this accident induces an insurmountable distaste for nourishment; so that the little patients will perish from inanition rather than attempt to take food. I have tried every means to remedy this difficulty. The best plan is to forbid liquids. I give my patients thick soup, of vermicelli usually, directing that they shall be fed with a fork. I allow, also, hard-boiled eggs, omelettes and rare meat, and interdict drinks of all sorts. If the thirst is insatiable, I allow a little cold water, and take care that it shall be given immediately before or a long time after food has been taken, so that vomiting shall be avoided. The complication of which I speak is rarely observed until three or four days after the operation, and does not persist commonly after the tenth or twelfth day; although I have known it, on one or two occasions, to last longer.

It would seem as though the laryngeal orifice, when it so readily admits liquids, should likewise permit the passage of air enough for the purposes of respiration; but it is not so. The canula may be removed, but dyspnœa will supervene.

It is difficult to account for this accident. Dr. Archambault believes that a child who has breathed through a canula for several days loses the power of consentaneous motion in the muscles of the pharynx and larynx, and speaks of an ingenious practice, which he has found useful, which consists in closing the canula momentarily at the instant of swallowing, and thus compelling the patient to make use of the larynx, whereby the normal muscular actions are restored. This little stratagem succeeds sometimes, but in many cases it fails completely.

This is what I have to say of tracheotomy, and of the small matters on which its success depends. What I have said has been, for the most part, already promulgated through the journals, or in my lectures at the Faculty of medicine; but the simplest and plainest things are precisely those that are hardest to teach, and the soonest forgotten.

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ART. IV.—*Treatment of Aneurism by Compression.* By DR. PAUL BROCA, Agrégé à la Faculté de Médecine, Chirurgien du Bureau Central des Hôpitaux, Paris. Translated by St. Geo. Peachy, M. D. Richmond.*

Gravity of the Treatment of Aneurism by Ligature.

THE substitution of the simple ligature in place of opening the sac in treatment of aneurisms was, perhaps, one of the greatest advances made mention of in the annals of surgery. The ancient mode of treating large aneurisms was so terrifying that only the most experienced operators dared undertake it, and so disastrous were its results, that many eminent surgeons, among whom it suffices to cite Palletta, Wilmer, Deschamps, Lassus and Percival Pott,† renounced this murderous operation, and gave preference to amputation.

When John Hunter's operation by simple ligature, an operation so easy, so efficacious, and, relatively, so innocent, triumphed over the barbarous one of more ancient date, those who realized this immense progress were led for a while to believe that the surgery of aneurism had finally arrived to a degree bordering perfection.

The modern method—such was the appellation given for some time to the ligature placed between the heart and the aneurismal sac—not only succeeded in diminishing the mor-

* The object of the translator is to present to the American public a succinct history of the various treatments employed heretofore in cases of aneurism. Such history, he believes, has never been given in a form more philosophically arranged than the one adopted by Dr. Paul Broca of Paris. He simply asks that awkwardness of expression, consequent upon the conversion of one language into another, be pardoned him, and that the pamphlet which will be published, after the separate numbers shall have been exhausted, be allowed to fill the vacuum which has too long existed in surgical literature and practice. To say that Dr. Broca is author of the work, is sufficient introduction to the American reader, by whom Dr. B.'s writings are always justly admired for their profound reasoning, deep research and clear order of arrangement. ST. GEO. P.

† Pott's Chirurgical Works, edit. Earle, London, 1808, vol. 3, p. 220; Scarpa on Aneurism, French translation, by Delpech, Paris, 1809, p. 269; Lassus' Chirurgical Pathology, Paris, 1805, in 8vo. vol. 1, p. 363.

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tality after operation, to a considerable degree, but it possessed the further advantage of curing certain aneurisms before which the ancient method was compelled to recoil. The history of the goldsmith upon whom Guattani operated for an inguinal aneurism is never cited save with a sort of dismay.* And we know that Desault having dared attack an aneurism of the axillary artery, suffered the pain of seeing his patient perish from hemorrhage before ending his operation.† The new method suddenly enlarged the domain of operative medicine—it offered favorable chances of cure in cases until then deemed hopeless. In addition to this, in consequence of the progress made in *anatomy of relations*, the execution of the ligature had acquired an admirable precision and brilliant rapidity. The legitimate pride of surgeons united itself then to the real interest of the patient, in order to assure the reign of the modern method; and such was the importance attached to it by all, that France and England disputed and still dispute the honor of having brought it to light.

Surely, we do not wish to depreciate the immense services rendered by the ligature—practiced according to Anel's or Hunter's method—to humanity the half century past; and it is probable that, had we, like M. Roux, assisted in the last disasters of the ancient method, we would have shared his just admiration for the ligature. But *we* commenced our career at an epoch in which the ligature was without rival, and, instead of appreciating its results relatively, we have appreciated them in an absolute manner. Now, if the ligature gains in being compared with the ancient method, it loses much, at least, in being examined in itself. In fact, statistics show that, at this day, it constitutes one of the most serious operations of surgery.

Among the facts figuring in Lisfranc's tables, we find 128

* Guattani, de externis aneurysmatibus, Rome, 1772, Histor. 15.

† Bichat, works of Desault, vol. 2, p. 555, 1st edit.; Boyer, Mal. Chir. vol. 2, p. 189, 1st edit.

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of them relating to aneurisms treated by Anel's method. In this number there were 36 deaths—i. e. 28 per cent.*

Again, Benjamin Phillips has collected 389 cases of the same kind, which resulted in 112 deaths—still 28 per cent.†

Finally, in about 600 cases of arterial ligature practiced for different causes, Porta found that the mortality has been more than 27 per cent.‡

These figures, we see, accord sufficiently well the one with the other. They do not differ even notably from those of M. Roux, who, in 38 cases operated upon by himself, lost 10 patients—i. e. 26 per cent.§

It is clear that these statistics, bearing at the same time upon all the arteries of the body, might conduct to erroneous appreciations. To avoid all confusion, let us take the operations practiced upon one artery alone—the femoral, for example—in a case of aneurism treated according to the *méthode d'Anel.* The results will not be more encouraging.

Dr. Norris of Philadelphia, to whom the science is indebted for abstracts so important of the results of surgery in general, and of American surgery in particular, has published a total of 188 cases of ligature practiced upon the femoral artery, after the *méthode d'Anel*, for aneurisms of the lower limbs. In this number were included 155 popliteal aneurisms—forty-six patients—i. e. twenty-four per cent. died from immediate results of the operation. The rest survived, six of them, however, remaining mutilated by amputation of thigh rendered indispensable. I omit mentioning several patients who had partial gangrene of foot, and place them in the catalogue of successes. But, logically, the amputated should be placed in the number of failures of ligature, for it is presumable that the surgeons did not decide to practice

* Lisfranc, Thèse de Concours, Paris, 1834, on the Obliteration of Arteries, &c. p. 130 to 149.

† Cited by Arnott, in Hunterian Oration, Feb. 14, 1843, London Medical Gazette, 2nd series, 1842–3, vol. 1, p. 749.

‡ Luigi Porta, on the Pathological Alteration of the Artery by Ligature and Torsion, 1845, p. 136.

§ Bulletin of the Chirurgical Society, 1852–3, vol. 3, p. 90.

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amputation, except to prevent an impending death. The ligature then failed about 28 times in 100.*

Benjamin Phillips' table is still more sombre, for out of 113 operations, not less than 36 terminated fatally—which makes 31 per cent. mortality.

These figures speak for themselves; and to him who knows with what haste successes are published and with what facility failures are forgotten, it is probable that this estimate falls yet below the reality, and that the necrology of the ligature, in cases of popliteal or femoral aneurism, goes perhaps beyond one-third of the cases operated.

An operation attended with such results cannot be considered as the last effort of the science. Hence, how many modifications has it been forced to undergo? How many researches, how many attempts have been made, during a half century, to substitute, in place of the ligature, operations of less gravity? Refrigerants, direct compression, compression upon the entire limb, (means of the last century,) have been tried anew; coagulation of the blood in the aneurismal sac, by means of heat,† by electricity, by acetic acid injections,‡ by alcohol,§ by perchloride of iron, have been assiduously brought to bear. It is sufficient to witness the unsettled convictions of surgeons of the present day, in regard to the question of aneurism, to be definitively convinced of the unsatisfactory results of the ligature. While the second chirurgical school of France, inspired by Pravaz, experimented in galvano-puncture and coagulating injections successively, another school of surgery, which occupies in the British isles a position at least equal to that

* George Norris, statistics of mortality following the operation of tying the femoral artery. American Journal of Medical Sciences, Philadelphia, 1849, Oct. p. 324.

† Everard Home, on the Coagulation by Heat of the Fluid in an Aneurismal Tumor. Philosoph. Transact. 1826, part 3, p. 189. The operation consists in coagulating the blood by introducing one end of a long acupuncture needle into the sac, and heating the outer extremity to whiteness.

‡ See in relation to this means, Wardrop's remarkable article in the Cyclopædia of Practical Surgery, conducted by Costello, 1841, vol. 1, p. 218.

§ Leroy (d'Etiolles,) Recueil de Mémoires, Paris, 1845, p. 281.

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occupied by Lyons in France, attached itself strongly to the perfection of the method by compression, and from it obtained remarkable results.

It is this latter method which we propose to make known in the present work. At the same time, however, let the reader be cautious in concluding from this fact that we have lost all hope of seeing one or the other of the Lyonese methods sooner or later triumph.

To us the idea of coagulating, by direct action, the blood contained in the aneurismal sac, is not one of those fleeting illusions to be found at every step taken in the history of surgical art. It is an idea perfectly rational and entirely scientific, and we are convinced that a brilliant future awaits it.

Now, that the ancient operation of opening the sac has justly fallen into desuetude, and that pathological anatomy has victoriously refuted the old hypotheses in regard to aneurismal cure, all agree in recognizing the fact that this cure can only take place by one sole mechanism, viz: by coagulation of the blood contained in the aneurismal sac. To obtain this coagulation by means the most sure, simple and prompt, is the end proposed by all modern methods; and when we reflect upon the rapidity, the facility with which blood outside of its vessels coagulates, we are truly astonished at the resistance which the same fluid has, up to this day, offered in the treatment of aneurism, to the action of physical or chemical agents, the best suited, in appearance at least, to produce such coagulation. We never think without regret of the galvano-puncture, that other conception of Pravaz—a conception so logical, so simple, and at the same time so ingenious. We ask ourselves if it really merits the oblivion into which it seems to be upon the point of falling; and we propose to publish soon a case of remarkable success furnished by it under the management of M. Voillemier. As regards chemical agents, we cannot habituate ourselves to believe that, among the innumerable combinations of matter, there may not be a single one pos-

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sessing these two properties, viz: of not irritating the walls of the aneurismal sac and of coagulating with certainty its contents.

Coagulating injections, as might have been expected, have not met with good fortune before the academy of medicine. Doubtless the results of their use have so far proved more disastrous than those of the ligature. The question, then, such as proposed by M. Malgaigne, has received the most natural solution in the actual state of affairs.

But the injections of the perchloride constitute only a special application of the general principle of coagulation by injection, and the majority of objections urged by the orators of the academy apply much more to the former than the latter. Possibly, then, the process may have been bad while the principle remains good. M. Malgaigne has wished to recognize this fact himself; and for our part, we dare believe that sooner or later some new liquid will realize the hopes to which the perchloride has given birth. It is very permissible to wish for the success of a method which would diminish the danger of the treatment of external aneurisms, would abridge their duration, and cure perhaps certain aneurisms before which surgery, so far, has retreated, or, certain others which it dare not attack without exposing the patient to fearful hazards.

We desired to place ourselves upon the reserve in regard to the Lyonese method, before entering upon the method by compression which has been so eminently successful in Dublin, London and America.

We now commence the study of this latter method, by

1. Giving a general idea of it.

2. By searching the history of the art to discover the principles upon which it is based, and we shall conclude, by

3. Describing precisely the *manuel opératoire* and the perfected instruments which can alone conduct to advantageous results.

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ART. V.—*Some Additional Remarks on the "Vaccine Question."* By WM. W. PARKER, M. D. Richmond, Va.

IN concluding my communication in the last number of your journal, on the "Consequences of Vaccination," I stated that its "length with other reasons" precluded comment at that time. It was thought best by others that the paper should appear in connection with the report of the committee to the legislature; and owing to the pressing nature of my engagements at that particular juncture, it was not prepared with that care which I desired. This much I desire to say, as an explanation of certain imperfections in the communication.

I propose at present to correct these defects as well as may be, and to make a few additional remarks on some of the facts stated.

In the foot note to case No. 6, (see table in last number,) I say, "These were either vaccinated from scab No. 1, or with matter gotten from the agent." Now, it is important to know what was the result of the use of the agent's virus in case No. 2, in which I know it was employed. This information I desired to obtain before the publication of my former paper, but the family in which it occurred having removed to some unknown locality, and being of very low condition, I could not find them out. I have, however, within a few days past, made the discovery by accident. I find that I was mistaken in my previous statement of this case. Instead of using my own virus *first*, I employed that of the agent. Mrs. Williams, the mother, says that I told her, when I found the agent's virus had not taken, that I would get some of my own and vaccinate her children; and that I did call some eight or ten days thereafter, and insert the matter. This "took," and was accompanied with no eruption whatever. She says she returned me the scabs. This statement is doubtless true. It is very evident, upon reflection, that I could not have procured virus, except from

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vesicles, in *thirteen days* after the vaccination in case No. 1. This statement also explains the fact that I had no matter when she first called upon me, but in 10 or 12 days thereafter, (23 days after the vaccination in case No. 1,) I would very naturally have virus.

The obvious aim of this enquiry is to trace, if possible, the source of the spurious virus. Now, does the failure of the agent's matter in these two children prove that it was altogether inocuous? It certainly has this appearance; though it is by no means conclusive evidence of the fact. I have frequently met with persons who say that they were *several times inoculated* in the "old country," without effect. In such cases the activity of the virus was undoubted. If, however, I did on other occasions use the matter from the agent, gotten through Mrs. Williams, it was most probably in cases Nos. 3 and 4, where no eruption occurred. While, then, these facts may prove the inertness or genuineness of the agent's virus, they also establish the genuineness of my own virus, gotten from No. 1, with small-pox in the house.

The first case in which I can find any eruption whatever occurring in my own vaccinations, and in which I afterwards used the virus, is that of No. 7. This was a healthy German infant, and the only exposure to small-pox to which it was subjected (unless I conveyed the contagion in my clothes) was the occurrence of a case of the disease some 200 yards off, a short time previous. This scab I valued highly, because the child was remarkably robust, and as stated before, I vaccinated some dozen persons with it, and gave portions of it to two medical friends, in whose hands it produced varioloid.

From some of the peculiarities of the recent epidemic, it is not difficult, it seems to me, to see how any physician might have come into the possession of spurious virus.

For instance, I vaccinate a child to-day, with pure virus, who has been exposed in some way to small-pox contagion. Eight or nine days thereafter I call, as is usual, to see the patient. I find the vaccine disease running its usual course.

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On my departure I request the scab to be sent to me. Two or three days after this visit a slight eruption takes place, followed by immediate subsidence of the constitutional disturbance. It being perhaps the first case of vaccination performed in the family, the parents feel no uneasiness whatever. They regard the "breaking out" as rather favorable. A fortnight afterwards the scab is left at the doctor's office, perhaps in his absence, and nothing is heard of the eruption.

That this may have been the real history of many cases, I have no doubt. What is singular is, that the eruption on the body and extremities does not run its course *pari passu* with the vaccine scab upon the arm. It was very early established, that if a patient were vaccinated to-day, and three or four days hence was again vaccinated, (in both cases with pure virus,) the latter vaccination would soon overtake the former, and both run their course together—the second scab being merely smaller than the first. But we now see cases where very different phenomena occurred. The case of the children of Rev. Mr. W. is in point. The eruption in the youngest child occurred after I ceased to visit the oldest. Besides, a friend of mine informed me some weeks ago that varioloid eruption followed vaccination *fourteen days* after its employment in the case of an old lady under his care. Another medical friend from the country tells me that he procured virus from a friend in this city, (not the agent, nor myself,) last winter, and with it vaccinated three negro men in a neighborhood where small-pox had prevailed, and that *eight days after the maturation* of the pustule on the arm, these men broke out with real small-pox—one a confluent case.

Another peculiarity of the recent epidemic is the exceeding mildness of many cases of varioloid. I am at present attending an infant who took the disease from its father, the disease being modified of course by vaccination. While speaking to the mother a few days since of the extreme mildness of some cases of varioloid, she exclaimed, "may be

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Molly has it;" and upon calling the girl from her play in the garden, (she is about 11 years old,) I discovered four or five papulæ upon the face and as many upon the neck. There was but one, however. and that upon the chin, that presented the hard, horny base, and that in no remarkable degree. This girl had not complained of being sick, and but for the presence of small-pox in the house, would never have been suspected of having varioloid. I have now under my care a boy eight years old, with measles, accompanied with slight pneumonia of the right lung, upon whose face, on the fourth day of the eruption of measles, appeared pimples, about a dozen in number, differing in no appreciable respect from those upon the face of the girl above mentioned. This child has been subject to a papular eruption on the face.

Dr. Watson divides varioloid into three classes, and many of the cases I have seen belong to the third and most infrequent variety. He says, "there are other cases in which a great part of the eruption consists of red pimples, which soon become livid, but contain, from first to last, no fluid whatever." I have seen cases in which not only a "*great part*," but *all* the eruption consisted of red pimples.

Another and the last peculiarity I shall mention is that in some cases the pustules fill up very rapidly. I attended recently a lady 55 years old, for varioloid, who was *inoculated* in childhood. Many of the pustules on the neck filled up in *thirty-six hours* from their first appearance. These pustules were very small, and at first produced some doubt as to the nature of the disease.

I wish to make a further statement in regard to case No. 15. I was very careful in my enquiries in regard to it, and those who have read my article in the preceding number of this journal, may remember the positiveness of my language on the subject. Meeting a few days since with the sister of the child, who took small-pox, and was sent to the hospital, I interrogated her (it being my first opportunity) as to whether her sister Emma had any breaking out whatever. She said she lodged with her, and that she noticed one

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"bump" on her forehead in the edge of the hair, and several upon her legs. It will be remembered that the oldest girl was attacked with the small-pox *twenty-four days* after Emma's vaccination. If we deduct eight or ten days, the time of the appearance of this trifling but important eruption, from the 24 days, we will have the ordinary period of incubation in the sister's system. This, I think, fixes the nature of Emma's eruption.

The other young lady, who was vaccinated at the same time and with the same matter with Emma, says she also had one "bump" on her head, but did not dream that it constituted an *eruption* in the sense suggested to her by my previous enquiries. This is certainly a very natural opinion.

These new facts of case No. 15, may abate the force of the conclusion drawn from it in my former paper, viz: that there is really no absolute test of the genuineness of any given scab, except a trial. I can, however, well imagine a case in which the detection of such "bumps" as were observed in the two persons above mentioned might escape the most careful examination of the physician.

LECTURES AND TRANSLATIONS.

Lectures on Varicella, Variola, and Variolous Diseases. By Armand Trousseau, Professor of Clinical Medicine to the Faculty of Paris, at Hotel-Dieu, &c. &c. [Translated from l'Union Médicale.]

*Lecture III.—Varioloid, (continued,) Vaccinia.**

Varioloid has only been known to the medical world in the last quarter of a century; that is to say, only since those memorable epidemics of small-pox which ravaged Europe about the year 1820. When you read in Sydenham, (for, to

* See No. ii, p. 136, No. iii, p. 236, for the two preceding lectures.

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study the history of epidemics, and particularly the history of small-pox epidemics, you must turn to Sydenham's masterly work;) when you read the narrative of the epidemics of variola of 1663–4–5, in Sydenham, you find nothing that recalls varioloid to your mind. When you read the history of epidemics of *anomalous* variola, you imagine you detect traces of varioloid, until you examine more attentively, and perceive the anomalies appear rather in the general phenomena, in the cerebral, pulmonary, or abdominal symptoms, than in local phenomena connected with the eruption. Sydenham, then, gives no clear indication of the existence of varioloid in his day.

Borsieri, who lived during the reign of inoculation, who himself inoculated, and saw inoculations practiced daily, Borsieri describes a *variola cornuta*, or horn pock, in which the pustules dried, and became hardened and tuberculated in a short period. This is something like varioloid.

At the Small-pox hospital of London, which Gregory directed early in the present century, and at the head of which he continued until 1853, when he died, this careful observer noted, even in 1815, 1816 and 1818, before there was any talk of varioloid, cases of small-pox in persons who had been vaccinated, inoculated, or who had already suffered one attack of variola, and he perceived that, in all these cases, the eruption was strikingly modified.

Finally, in the last twenty-five years, in consequence of the substitution of vaccination for inoculation and the degeneration of vaccinia, there have been several epidemics of modified small-pox, and varioloid has been carefully studied and clearly defined. If we compare this form of variola to that anomalous small-pox, which prevailed epidemically in old times; if we compare it, especially, to the anomalous variola described by Sydenham, we shall be convinced that varioloid is a discovery of our own day—that it is simply variola modified by vaccinia.

Let us now pass to the history of vaccination.

The great Jenner was a skillful inoculator in Gloucestershire, a fertile country, abounding in meadows and droves of cattle. From 1792 to 1798, when every body in England was inoculated, Jenner observed several instances in which individuals were refractory to inoculation. He was struck by these facts. He endeavored to account for them for a long time, without arriving at any satisfactory conclusion. At last, he was told by a neighboring farmer that persons who caught the pox from cows never had small-pox. On examination, he found that this cow-pox was a pustular

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eruption on the teats, which sometimes communicated a similar eruption to the milkers. He learned that it was traditional among the dairymen, that persons who had once contracted this kine-pox never had variola. This was a ray of light to him. He visited all the farms in his vicinity, enquiring into the subject, and everywhere this fact was corroborated. Jenner then conceived the marvelous idea, that by inoculating man with cow-pox, he might be protected from variola. To realize his idea, he resolved to make the experiment. He did not take matter from the cow's teat, but from a pustule on the hand of a milker suffering from kine-pox, and he introduced this virus into the arm of a boy. The inoculation was successful—*vaccination was discovered!*

But kine-pox was a strange, incomprehensible disease, occurring but rarely in the cow, and its cause was completely unknown. Researches were undertaken to discover the origin of this singular affection. Jenner was told that it was the result of a virus transmitted from the horse to the cow. The disease of the horse from which vaccinia was supposed to originate, was what is termed *the grease*, or, in French, *eaux-aux-jambes*, a kind of vesicular and pustular eruption. The country people said to Jenner: if we milk our cows after grooming horses which have *the grease*, the former have cow-pox.

In 1801, Dr. Loy attempted to clear up this subject, by inoculating cows with *the grease*. He was unsuccessful, but he suspected that the cause of his failure was that he did not inoculate early enough. He had already discovered that the pus of cow-pox, taken from the teat at a certain period of the disease, produced no result on inoculation. He inferred, from analogy, that if instead of waiting until *the grease* became chronic, he inoculated from its pustules in its early stage, he might get a positive result. He did so, and arrived at the conclusion that the pus from the pastern of the horse produced genuine kine-pox when inoculated on the teat of the cow, and that this kine-pox could be transmitted to the human subject in the usual way.

Sacco repeated these experiments on 27 cows and 8 children, with negative results. Subsequently, in an experiment conducted with greater care, he succeeded in inoculating a cow with matter taken from a pustule of *the grease*, and, from the pustule on the teat of the cow, he obtained a true vaccinal pustule on a child's arm.

These experiments were successfully repeated by a Danish physician.

Thus, according to these experimenters, a disease trans-

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mitted from a soliped to a ruminant, and from a ruminant to man, possesses the marvelous property of preserving the latter from variola, a disease of the human subject.

The great majority of physicians, however, discredited this origin of kine-pox, and considered the experiments I have reported as apocryphal, because they failed in attempting to repeat them.

We now know positively that kine-pox is developed in cows without any communication with horses. When the government of Wurtemburg undertook to regulate the practice of vaccination in its territory, it offered a reward of a thaler, and afterwards of two thalers, to any one who would make known the existence of kine-pox among his cattle, and a reward of four thalers for every direct inoculation from the cow. At that time, an epizootic of kine-pox was prevailing in that country. The commissioners appointed to examine the animals that were reported to have the disease, found that a large number had a false pox, but a considerable number were really affected with vaccinia, and furnished pus which produced a legitimate vaccine pustule in the children on whom it was inoculated. It was satisfactorily determined that these cows had not had any communication with horses affected with the grease.

About this time, Dr. Macpherson, in India, inoculated healthy cows with matter from a generalized pustular eruption resembling kine-pox which he detected in several cows, and succeeded in his experiments. He then took pus from the sores he had produced and inserted it into the arms of children, and procured inoculable vaccine pustules.

Kine-pox had been detected, then, in Wurtemburg, as I have described. This was discredited in France, in accordance with the custom of our country to deny everything that we do not see ourselves. In 1836, however, a Madame Fleury, who kept a dairy at Passy, informed her physician, Dr. Perdreau, that her cow had the pox, and that she had contracted a disease from the animal. Dr. Perdreau observed that she had true vaccine pustules on her hands, and immediately informed the academy of medicine, which appointed MM. Bousquet, Emery, and Gérardin to investigate the affair. These gentlemen went at once to Passy, examined the cow and its mistress, and made sure of the existence of kine-pox in both. As the pustules on the cow's teats were desiccated, they took pus from the hands of Madame Fleury, and inoculated a number of children. It was thus that vaccinia was regenerated in France.

In 1845, Dr. Steinbrunner of Alsace, who, as well as his

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father before him, had long sought unsuccessfully for vaccinia in the cow, in 1845, this physician detected the disease, and inoculated children.

Cow-pox exists then; it has been recognized in Germany and twice in France, since the Jennerian epoch. But if this disease of the cow, transmitted to the human subject, protects the latter from variola, is it not probable that, primarily, variola was communicated to the cow by man, and its virus, being modified in the lower animal, produced cow-pox, which is the source of vaccinia? Such was the idea of many physicians at the very outstart of vaccination. They said, kine-pox is only a benign variola; the virus of small-pox implanted in a foreign soil is modified, and when thus modified it is communicated anew to man, it produces a disease which protects him from variola. It must be confessed that this was a very reasonable, or at least, rational idea; but was it true?

In 1807, an English experimenter inoculated 12 cows with variolous pus. He claimed to have produced kine-pox in one instance. But no one believed him.

A few years afterwards, Dr. Thiele, a Russian physician, attempted to inoculate cows with small-pox. He experimented on numerous animals unsuccessfully. At last, he was convinced that he failed from the imperfection of his mode of operating. He then made deep incisions in the teats of cows, and, in this way, obtained pustules. He pretended that the pus thus generated produced legitimate vaccinia in children.

A physician of Munich, who, for nine or ten years, had been trying, unsuccessfully, to inoculate cows with variola, employed Dr. Thiele's method with complete success.

Dr. Cely of England, published some analogous facts, which he corroborated in a letter in reply to some enquiries I addressed to him.

Lastly, a Danish experimenter took advantage of a small-pox epidemic to employ a different method. He enveloped cows in the bed furniture of patients who had died of small-pox, and pretended to have produced an epizootic of kine-pox by these means. This experiment was repeated by commissioners appointed by the king of Denmark, with negative results.

It was subsequently repeated by Dr. Miguel, of Tours, in France, under the following circumstances. Along the banks of the Loire, there are a large number of vine-growers, who live in caves in the rocks, like veritable troglodytes. There they are huddled together, men and beasts; the cattle

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in the back part of the cavern, usually; the men nearer the outer air. In these dim caves, frightful epidemics of variola occasionally break out. On one of these occasions, Dr. Miguel availed of the opportunity of repeating the experiments of the Danish physician. He left his patients pell-mell with the cows, and whenever one died, he covered a cow with the bedding of the dead man. He moreover inoculated the teats of the cows with variolous pus. But not a single cow contracted kine-pox or variola.

With these conflicting and contradictory statements and experiments, I think it impossible to decide, at present, whether vaccinia is, or is not, variola degenerated and attenuated by its transmission through the cow.

Attempts have also been made to inoculate the cow with vaccinia. It is very natural to suppose that we may restore to the cow, by means of vaccine, that kine-pox we took from her to make vaccine. For twenty years a demonstration of the possibility of such an inoculation was sought in vain. From 1809 to 1829, no authentic experiment was produced to prove it. But on the new discovery of kine-pox in Wurtemburg, it was found easy to vaccinate children from the cow and to take vaccine virus thus developed and produce kine-pox in other cows. After the discovery of kine-pox at Passy, Dr. Bousquet declared that it was a mere sport to produce kine-pox in cows with the regenerated vaccine matter. Now-a-days, when the matter has grown old and enfeebled by transmission, it is not only not a sport, but it is altogether impossible. It is important to observe that when the cow is successfully inoculated with vaccine matter, the resulting virus is notably diminished in power.

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CHRONICLE OF MEDICAL SCIENCE.

MATERIA MEDICA AND PHARMACY.

1. *The Muriate of Opium.*

We have met with, in looking over old files of the Medical Times and Gazette, a new preparation of opium; and as it is always desirable to have as many modifications as possible of a drug which so frequently requires to be used, and which acts so differently on peculiar idiosyncracies, we give it a place in our pages.

Take, of the best powdered opium, ℥i; muriatic acid, ℥i; distilled water, ℥xix. Shake this mixture frequently for fourteen days, and strain. The dose is from twenty to forty drops.

2. *The Adhesive Iodine Paint.*

The following is the formula for preparing an iodine paint, which is made adhesive by the addition of mastic, and is thus prevented from spreading too much when applied to the skin. As the value of this application in chronic inflammations of the eyelids is becoming recognized, it is evidently important that we should be enabled to apply the paint so as to prevent its spreading on the more delicate structures in the neighborhood. ℞ Spirits vin. rect. ʒij; spts. aeth. nitr. ʒiv; mastic, ʒss.; iodinii ad sat. This is the preparation used at the Moorfields ophthalmic hospital with great success as a counter-irritant.

3. *Chloride of Zinc Collyrium.*

Mr. Critchett of the Royal ophthalmic hospital (Moorfields) has been in the habit of using frequently a lotion of the chloride of zinc as a lotion in thickened and vascular conditions of the conjunctiva. He regards them as in an analogous condition to the gleets of the urethra, and calls this condition a "gleet of the eye." Its strength is one grain to the ounce.

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4. *Linctus for Chronic Cough.*

The following is the formula for the linctus in common use in St. Bartholomew's hospital:

℞ Conf. rosæ caninæ ℥ij; tragacanth. pulv. ʒi; syrupi papaveris ʒvj; aceti scillæ ʒvj; acidi acetici, Mxx; aquæ ferventis ℥vj. Misce. Dosis ℥i ad ʒiij.

A less expensive preparation is used for the same purpose in very large quantities at the London hospital, under the name of Lohoch:

℞ Sacchari fæcis ℔xxviij; ol. olivæ ℥viij; farinæ tritici ℔iv; tinct. opii.; aceti scillæ, ä ℥viij. Misce. Dosis ʒi ad ʒiij.

There are many forms of chronic cough, both in children and in adults, which are treated much more successfully, and far more agreeably to the patient, by the use of preparations of this kind than by the commonly prescribed "cough mixtures."

5. *Eau Sédative.*

We obtain from the Medical Times and Gazette the two recipes for the famous Eau Sédative of the French pharmacopœia.

1. ℞ Camphor, 30 grammes (℥i); dissolve in alcohol at 22°, 250 grammes (½℔); add liquid ammonia, 60 grammes (℥ij); anise oil, eight grammes (ʒiv).

This mixture is to be inhaled, and to be applied on a piece of cloth to the forehead, in cases of sick headache.

2. ℞ Liquid ammonia, 100 grammes (℥iij); common salt, 20 grammes (ʒx); distilled water, 900 grammes (nly. 2℔); camphor, two grammes (60 grs.); essence of roses, a few drops.

This mixture is to be applied on the forehead like the preceding.

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *Carbonic Acid Gas as an Anæsthetic.*

A very interesting paper, read before the New York Academy of Medicine, from the pen of Professor Simpson of Edinburgh, treats of the anæsthetic influence of carbonic

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acid gas, especially when applied to mucous surfaces. The author believes that the "burning herbs," used in the days of Hippocrates, owed their efficacy to the carbonic acid evolved, and not to any medicinal quality they possessed. So also with regard to the medicinal virtues of the mineral water baths of Germany, which contain large quantities of this gas. It is also probable that the old "yeast poultice" of the Lady Bountifuls of our country may relieve pain through the agency of the carbonic acid gas which they generate.

After alluding to the anæsthetic property of this gas when applied to the cutaneous surface, as well in its natural state as when the external cuticle has been removed by a blister, the professor remarks that this property is still more highly marked when applied to a mucous surface, and he has found it exceedingly useful in neuralgia of the uterus; and in almost every disorder of that organ attended with pain. He has applied it daily for months in some cases, and has sometimes found its use to result in a cure of some diseases. He finds it to be preferable to the vapor of chloroform, as a topical anæsthetic, to the genital mucous canals, inasmuch as the latter generally creates a disagreeable, and sometimes a very painful though temporary feeling of heat and burning. The injection of carbonic acid gas produces no such painful sensation. Several cases are given of its beneficial effects, not only in cases of neuralgia and irritable uterus, but in dysuria and irritability of the bladder, in bearing down pains of the uterus and neighboring parts, and even in malignant disorders of the matrix; he has also found it exceedingly useful in alleviating the pain, and improving the character and odor of the discharges, when such exist offensively. Prof. Simpson does not, however, claim the credit of originality in the suggestions of the use of this gas as a means of relieving local pains, but proceeds to quote several authors by whom it was employed more or less, though from some reason its valuable properties seem to have been overlooked. The late Dr. Dewees of Philadelphia, used vaginal injections of carbonic acid gas in the treatment of carcinoma uteri, with the happiest effect in alleviating the severity of the pain, and diminishing the offensiveness of the discharge. In cases of dysmenorrhœa it has been recommended by Prof. Major of Geneva, who used it two or three times a day for five or six minutes each time. As quoted by Dr. Rossi of Italy, Dr. Pereira found it to act speedily and beneficially as a local sedative in uterine pain.

In practice, Prof. Simpson states that he uses a common wine bottle, and forms the gas by mixing in it six drachms

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of crystallized tartaric acid with a solution of eight drachms of bicarbonate of soda in six or seven ounces of water. A long flexible caoutchouc tube, having one end fastened to the perforated cork of the bottle, conducts the gas to the vagina.

2. *Vaccinia and Variola in the same Patient.* By Dr. J. L. Abbott of Boston.

This case may be cited as one of many which could be collected to show the impossibility of modifying the vaccine pustule by a complication with small-pox.

We have recently observed—and shall report on another occasion—an instance of vaccinoid and varioloid diseases occurring in the same patient, an adult. Such cases are rare, and have an interesting and important bearing on the question of the protective power of vaccination.

"A nursing infant of Mrs. Q——, some eight or nine months old, was vaccinated by myself, after it had been exposed to the contagion of a mild case of varioloid several days. The operation was successful, two perfect vesicles being the result; and on the seventh day I took virus from the arm, and with it vaccinated two other children. On the day immediately succeeding, viz: the eighth, a papular eruption appeared upon the infant, which, as it developed itself, assumed all the characters of unmistakable small-pox. The eruption was very full, as full as possible without being confluent, and the disease went on to a fatal termination. The vaccine vesicles, perfectly normal in their character at the time that virus was taken from them, from that day ceased to follow the usual course. They became large, irregular, and flattened pustules, accompanying the variola in its development. The children vaccinated with matter from this patient had genuine vaccine vesicles, without any unusual constitutional disturbance or breaking out on the skin.

"The following points are particularly noticeable in the above case:

"1. The infant must have had latent variola at the time of vaccination.

"2. The vaccinia was able to establish itself locally to such a degree as to extinguish, at the points vaccinated, the latent disorder up to the eighth day.

"3. After this period, the variola overwhelmed and engulfed, as if were, the vaccinia, and was able to expend its full force upon the system of the patient."

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3. *The Connection between Bronzed Skin and Disease of the Supra-renal Capsules.* By Drs. ADDISON, BUDD, PEACOCK, and Mr. HUTCHINSON.

An observation recently made by one of the most accurate and profound investigators of our day, Dr. Addison of London, has led to a series of post-mortem examinations in the British hospitals, in order to ascertain whether there could be traced any connection between the peculiar discoloration of the integument called "bronzed skin," and disease of the supra-renal capsules. Dr. Addison having first suggested the probability of such a connection, has been active in prosecuting his researches. He has examined twelve cases of bronzed skin, finding in every instance marked disease existing in these bodies, and always accompanied by a cachexia, which was sooner or later fatal in its termination.

Mr. Hutchinson of London, Dr. Budd of Bristol, and Drs. Peacock, Rowe, Burrows, Sibley and Farr, of the London hospitals, are also bestowing much attention to this subject, which may result in valuable discoveries as to the physiological uses of the supra-renal capsules. Twenty-seven cases in all are reported, and no single case has yet been recorded in which a well-marked bronzed condition of the skin existed and death did not result, nor any in which the supra-renal capsules were not diseased; nor has any case in which after death disorganization of the supra-renal capsules existed without the bronzed skin being always the accompaniment.—*Med. Times and Gaz.*

4. *A New Method of removing Soluble Substances from the Œsophagus.* By Dr. ELLSWORTH.

It not unfrequently happens in practice, that we meet with portions of solid food lodged in the æsophagus for a considerable space of time, and producing always the most unpleasant if not serious consequences. The novel suggestion of Dr. Ellsworth of Hartford, in a late number of the Boston Medical and Surgical Journal, may be found worthy of future experiment, under such circumstances.

Dr. Ellsworth proposes, in these obstinate cases of impaction, to dissolve the bolus of food, and for that purpose he appeals to the gastric juice as the great solvent prepared by nature for such purposes. By killing a pig or some animal, the gastric juice can easily be obtained in a fresh state, and should be administered in teaspoonful doses every hour until

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the mass is sufficiently dissolved to admit of its being dislodged, either with the probang or by the efforts of deglutition. Three very obstinate cases are given in the article from which we prepare our abstract, where entire relief followed the use of this ingenious suggestion, after all other means had failed. The remedy should be continued for some hours, however, before we may obtain the desired relief.

5. *A Case of Small-pox in the Fœtus.* By M. Blot.

The following well authenticated case of small-pox occurring in the fœtus was reported by M. Blot, at a late meeting of the Societé de Biologié of Paris. We find it reported in the Gaz. Med. de Paris:

The mother of the little patient, previously in good health and six months advanced in her second pregnancy, was attacked with small-pox, on the 17th of July 1854. She had not been vaccinated, but the attack was not severe, and she recovered without any secondary fever. During her illness, the movements of the child were more continuous than they were before, but not so energetic; during her convalescence the same movements became more and more feeble, until they ceased altogether, and the child was felt to fall towards the side on which the mother happened to lay. Two days after this cessation labor came on unexpectedly, and the mother removed to the "Clinique d'Accouchements," where she was presently delivered of a male fœtus of the six and a half or seventh month, *covered with pustules of small-pox.* This fœtus presented unequivocal evidence of having been recently alive, and there was every reason to believe that it had had small-pox at the same time as its mother, and that it had died from this cause.

6. *On Uterine Asthma.* By Dr. Simpson of Edinburgh.

Dr. Simpson has reported several cases of uterine asthma connected with menstruation. He made a few preliminary remarks on the observations of some German physiologists, with regard to the quantity of carbon excreted from the lungs during menstruation. Dr. Simpson had seen several cases of asthma, where the spasmodic attacks recurred at each menstrual period. In some, the difficulty of breathing was very great indeed; some, so much so, that in one case in particular, the breathing could be heard from any part of the house; in other cases, where the catamenia were scanty, but present, the attacks were not so severe, the dyspnœa

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slight, but still recurring at the periods. Dr. S. mentioned the particulars of several cases, and stated that the dyspnœa was completely relieved by chloroform. Dr. S. wished the other members to assist him in investigating this subject.

Edinburgh Med. Journal.

[A similar case was reported in this Journal, by Dr. Hoyt, which was cured by the iodide of potassium. See vol. 53, p. 326.—*Editors Boston Med. and Surg. Jour.*]

OBSTETRICS.

On the Distribution and Relative Proportion of Sexes in Multiple Gestations, and the Influence of Inheritance in the Production of these Gestations. By M. Baillarger. (Nashville Jour.)

M. Baillarger presented to the Academy (of Medicine) a paper which he read to the Academy of Sciences at its meeting of the 26th of November last.

This paper has reference to the distribution and relative proportion of the sexes in multiple gestations, and the influence of inheritance upon their production.

I. *Distribution of the sexes.*—The facts under this head are arranged into three groups, comprehending

First, the association of two boys.
Secondly, the association of two girls.
Thirdly, the association of one boy and one girl.

The following are the results obtained in this respect in 256 multiple gestations:

Two boys, - - -	100
Two girls, - - -	58
One boy and one girl, -	98

It is thus seen that the association of two boys in twin gestations, is almost twice as frequent as that of two girls. It is also seen that the third group, (that of the association of the two sexes,) is nearly equal to the first.

II. *Relative proportion of the sexes.*—The solution of this question is found in the figures indicated above.

In 512 twin infants there are found:

Girls, - - -	214
Boys, - - -	298

The number of boys then exceeds that of the girls more than one-third. This will appear very remarkable if it be remembered that in ordinary births the proportion is 16 girls

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to 17 boys; thus showing a difference in the one case of one-third, whilst in the other there is only a difference of one-sixteenth.

In twin gestations then, the relative proportion of sexes is governed by special laws, and they are entirely different from those acting in normal births.

This fact, interesting in itself, becomes the more so if compared with the facts already called by M. Flourens, upon the relative proportion of the sexes in lower animals—facts that show the predominance of males over females to be from $\frac{1}{16}$ to $\frac{1}{6}$. I ought further to remark that the very great predominance of boys over girls in twin gestation is related to another fact found in the general statistics of births. I allude here to the large excess of boys in the number of still-born children—the proportion being 17 boys to 12 girls. This singular preponderance of boys may to my mind be partly explained upon the ground of the predominance of boys in twin gestations, which is well known to be quite a large contingent to the statistics of still-born children.

III. *Influence of inheritance.*—Twin gestations are hereditary in certain families, but in various degrees and in different conditions.

A large number of facts prove that the daughters of mothers who have borne twins, quite often themselves become mothers of twins. This tendency sometimes skips over a generation, and it is the grand-daughter that becomes the subject of one or more twin gestations.

The facts that I have collected would tend also to prove that this hereditary disposition is also transmitted through sons.

Certain men possess the power of procreating twins without there being any hereditary disposition in the woman to twin conceptions.

This latter proposition would possess great importance in a physiological point of view, and I am fully aware that it should be supported by indisputable testimony; I will therefore only now announce it, promising, however, to return to it in a future paper. Before concluding, however, I may refer to the fact that this hereditary disposition just noticed has been profitably taken advantage of, in obtaining animals that would bring two instead of one young. In this way flocks of ewes have been obtained that normally carry two lambs—so constantly, that the single gestation became the exception instead of the law. I have seen a herd of nearly one hundred sheep of which every ewe bore yearly two

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lambs. Now the putting of the rams of these flocks to ewes that bring but one young might solve the question of the influence of the male over the number of the young at a birth.

I hope soon to see this experiment made, and will hasten to make known the result to the Academy.

Gaz. Med. de Paris, Dec. 1, 1855.

BIBLIOGRAPHICAL RECORD.

I. *Manual of Chemical Physiology, from the German of Professor Lehmann.* Translated, with Notes and Additions. By J. Cheston Morris, M. D.—with an Introductory Essay on Vital Force. By Samuel Jackson, M. D. Professor of Institutes of Medicine in the University of Pennsylvania. Philadelphia. Blanchard & Lea. 1856. (Received from J. Woodhouse.)

The book before us was originally designed by its author as an abridgment of his larger work on "Physiological Chemistry," which first appeared in the English language, as translated by Dr. Day of London, and but recently has become familiar to the American profession in the edition brought out by Messrs. Blanchard & Lea, under the supervision of Professor Rogers of the University of Pennsylvania.

In its more concise and simplified form, this "Hand-book of Physiological Chemistry" gives the student an opportunity of examining the vast and laborious researches of the German physiologists in the field of Zoo-chemistry. We can but admire the patient labor exhibited on every page. With what careful analysis has every one of the organic compounds been tested by the chemical forces—how carefully and yet accurately have all the chemical laws been brought to bear upon the living tissues of the human body. The histo-genetic substances, the protein bodies, and the animal fats have been reduced to their ultimate principles; the secretions of the

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human body have been scanned by the eye of the microscopist, and tested in the crucible; the solids of the human body have been tortured by every art which could be appealed to, and asked to give up their secrets, and the conclusions arrived at by the learned author may be summed up in the following sentences:

The same chemical laws preside over the constitution of different compounds, whether organic or inorganic.

All the differences observed between these two different classes of bodies are accidental, relative, and *have nothing essential.*

There is no such thing as *vital force;* the wonderful phenomena displayed in the germination of seeds; the modifying of a few simple chemical elements into the complex creations of the living organism are positively denied, and there is no other difference between the breathing, thinking, intellectual man and the clod of the valley, save in the complicated structure which his more elaborate chemical constitution has developed.

It is well when science leads us to such conclusions as these, and more especially when such works are placed in the hands of the young at an age when the mind is prone to delight in bold speculations and original theories, that there are those amongst us who are able to confute such material doctrines and to point out the errors to which too much learning oftentimes leads. We are, therefore, the more pleased to notice the admirable introductory of our own *Jackson* attached to the American edition of this work. We are sure that he never gave us more pleasure than when he manfully defends the doctrines of vitalism, and in his own brilliant and eloquent language refutes the heresies of the chemical school.

In the infinitude of the phenomena and forms and beings that compose the organic world, the universality and dependence of causal connections and special law, and in the evidences of its ever-during beauty, order, and harmony that pervade organic nature, is demonstrated the supremacy of a

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dominant force, far transcending the physical forces in the higher nature of its productions, and more exalted character of its powers, influences and relations. In its operations it appears as a ray emanating from the divine intelligence, stamping on each created living being types of organic structures, a revelation in material forms of the creative ideas of the Eternal Artificer of the universe. That force cannot be more appropriately expressed, than as the organic or vital force. This conclusion will no doubt be condemned as unscientific by physicists generally. In the present state of knowledge, the inexplicable and inconceivable in the physical, and still more in the physiological sciences, constantly cross our path, and arrest our investigations. Knowledge, science, philosophy, have their *ultima thule*, beyond which lie the vague and dreamy realms of conjecture.

The sciences are the facts or phenomena discovered in the order of their connection, and interpreted by man's intelligence, such as they exist in the world exterior to itself. Indissolubly connected, they form the circumference of a vast circle, each advancing as it progresses, and is developed like radii, and meet in one common centre. That centre is the incomprehensible, the infinite; it is the first Eternal Cause—the creative God!

The argument urged as conclusive by Professor Lehmann in the defense of his chemical theory of life is the following: "We should not lay down a new force, a general specific cause, until we have eliminated all other possibly operative forces from the group of phenomena in question. A proof of the existence of a purely vital force is only to be obtained by an exclusion of every physical force." As answer to this argument we quote Dr. Jackson, p. 43:

A full assent is given to the conditions of the argument; and it is affirmed most positively that the only phenomena—the exclusive attributes of a vital or organic force—the creation from a liquid formless plasm of typical organic forms, and organic instruments to execute physical, chemical, mechanical and dynamical actions, in the living organism, which are operative only during life, cannot be explained by any one, or all of the physical forces combined. They are absolutely excluded. Let the subject be expressed in special terms, and it is affirmed that neither heat, nor light, nor electricity, nor magnetism, nor gravity, can construct an

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eye, can form a retina, heterogeneous in its organic materials, complicated in the distinct organized anatomical elements of its structure, and endowed with a special sensibility—light and colors—manifested normally only when excited by a special external agent, the luminiferous ether. Nor can they generate the black pigment cells of the choroid coat, for the express purpose of suppressing the luminous rays, which have accomplished their impression, without which there can be no distinct vision; nor cause the formation anterior to the retina of the vitreous humor and the crystalline lens, admirably adapted in form, shape and density, to refract the rays of light into a focus, the distance from the retina calculated so truly, as to subtend on it a visual focus, in which is a perfect representation of external bodies. The formation of this organic optical apparatus, constructed in the fœtus included in the ovum or uterus, excluded from the direct operations of the physical forces except heat, it is affirmed, is absolutely inexplicable by the physical forces, or by any known physical process or action.

When we examine the discoveries and developments which have resulted from the immense labors of the chemical physiologists, we see on all sides the many important and valuable additions they have made to the science of physiology; and although we are willing to acknowledge that they have done much good in breaking down the bigoted and partial creed of the old vitalist by showing what a great part the laws of chemistry have to play in the animal economy, yet when they leave the laboratory and begin to mount upwards into the mysterious and unfathomable function of the living organism, we find them as much at fault and as incapable of explaining the wonderful phenomena every where surrounding them as the strictest and most dogmatic of the pure physiologists. A glance even at the work before us will satisfy the reader of the truth of this remark.

Nothing can be more complete than the description and chemical analysis of the greatest of the protein bodies, *albumen*, yet, when he begins his examination of its physiological importance, Professor Lehmann pleads his ignorance thus: "We know not, in the mean while, in what manner cells and tissues are formed from albumen." The pheno-

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mena of the living tissues are sealed books to his chemical tests, and he stands confounded before the mysterious workings of the vital forces.

Again—when our author treats of the process of digestion, the field wherein chemistry indeed plays a most active part, he declares that the food and the membranes surrounding it hold certain physical relations to each other, and that "absorption is nothing more than a function of the mechanical conditions just mentioned."

As, however, we have not reduced any of the different varieties of attraction between water and solid bodies to a mathematical formula, and the relations between them are not ascertained, we are still unable to give an actual explanation of the process of absorption; for, how is an explanation, *i. e.* a deduction of the phenomena from laws, conceivable, when these laws are still unknown? We must, therefore, at present, content ourselves with a general indication of the phenomena of absorption according to physical analogies, being confident, however, that physical conditions alone give rise to absorption.

A very imposing array of the chemical arrangements intended to facilitate the process of absorption is closed with the following words:

We find, therefore, peculiar mechanical provisions, also, for the acceleration into the blood of substances which are not easily diffusible; these consist of the *chyle-vessels.* We do not, indeed, as yet, possess a clear idea of the mechanism of absorption by the latter. All the experiments for the explanation of absorption by the lymphatics, have reference only to the onward movement of the fluid which has already entered the lacteals; the contractility of the intestinal villi, dependent on the smooth muscular fibres which enter into their structure, must naturally, from time to time, cause the fluid contained in them to be driven forward, while the valves in the vessels themselves prevent its return. But, how the minutest ramifications of the lymphatics are filled, and how it happens that a number of substances, while close to the blood capillaries, pass by into the extremities of the lacteals, we are entirely unable to explain.

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We might extend these remarks still farther, did not our space forbid; and indeed we are well satisfied to leave the readers of this work (and every one who is desirous to be familiar with modern physiology should read it) to determine, each one for himself, upon the important doctrines here taught. He may compare the purely physical theory of the great German with the animated and forcible argument of the American vitalist, and we doubt not that, whilst he may derive much that is valuable and instructive from the admirable investigations of Professor Lehmann, he will close the book, holding still firmly to his belief in the superiority of the wonderful machine, which is stamped with all the perfection of God's handiwork, over any composition of the laboratory, or any combination which chemistry alone can ever succeed in producing.

II. *Physical Exploration and Diagnosis of Diseases affecting the Respiratory Organs.* By AUSTIN FLINT, M. D. &c. &c. Philadelphia. Blanchard & Lea. 1856. (Received from G. M. West.)

Every one acknowledges the great importance of physical exploration in diseases of the heart and lungs, but few even of the younger members of the profession bestow upon this science that attention which it deserves.

This is in part owing to the want of hospital privileges, where a systematic course of pupilage is afforded. The ear must be tutored by patient and repeated efforts until it is enabled to distinguish between the various sounds, whether of health or disease, and give to each its proper value; and a long residence in some large hospital is absolutely necessary to give the experience requisite to be obtained by all those who desire to become accurate and dexterous auscultators.

We are therefore fearful that the very excellent work of Dr. Flint will be too often passed by and overlooked by the profession, who may spend their moments of study in the

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investigation of some more simple subject, and neglect the great science which his work is designed to teach.

Dr. Flint has brought together all the literature of auscultation in its connection with the natural and abnormal sounds of the lungs, and his labors have resulted in this handsomely printed and bulky volume of more than six hundred pages. We may imagine how rapidly have the facts and theories accumulated on this subject, when we reflect what a little time has elapsed since Laennec proposed this method of investigation; and we are glad to be able to appeal, as we can do with confidence, to this production to aid us in the acquisition of all the recent discoveries which have been made by the great auscultators of the European school.

We are always glad to welcome any work of merit emanating from one of our own countrymen, and especially when it is likely to reflect credit upon the American profession, and we hence cordially congratulate Dr. Flint on his valuable contribution to the medical literature of the day.

III. *A Practical Hand-book of Medical Chemistry.* By JOHN E. BOWMAN, F. C. S. Second American from the Third London Edition. Philadelphia. Blanchard & Lea. 1855. (Received from J. W. Randolph.)

We have too often referred with profit to the pages of this little work, to hesitate in giving it unqualified praise. Very unassuming in size and only intended "to furnish a plain and trustworthy text-book to medical students," it has passed through three English and two American editions in five years, and hardly requires us to spend much time in dilating on its merits when so many of our readers are already familiar with its contents.

This edition has many additional illustrations, and is of convenient size and moderate price. Amongst the numerous publications of Messrs. Blanchard & Lea, we know of none better worthy of the patronage of the profession than Bowman's Medical Chemistry.

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IV. *An Analytical Compend of the Various Branches of Medical Science for the Use and Examination of Students.* By JOHN NEILL, M. D. and FRANCIS GURNEY SMITH, M. D. A New Edition, Revised and Improved. Philadelphia. Blanchard & Lea. 1856. (Received from A. Morris.)

To compress the whole science of medicine in less than 1,000 pages is an impossibility, but we think that the book before us approaches as near to it as is possible. Altogether, it is the best of its class, and has met with a deserved success. As an elementary text-book for students, it has been useful, and will continue to be employed in the examination of private classes, whilst it will often be referred to by the country practitioner.

This edition is an improvement on the last; the pages are large, and much new matter introduced. If asked for an expression of opinion on the relative merits of the various divisions of this work, we would be disposed to select the treatise on physiology as being a very neat compendium of the fundamental principles of that science, and altogether a clever thing.

EDITORIAL AND MISCELLANEOUS.

European Medical Literature.

WE sometimes hear the remark from our most valued friends and faithful readers, that we do wrong in devoting so much space to translations and abstracts from foreign periodicals, to the neglect of our own more practical and useful literature. They do not deny the originality and brilliant theory of the French school, nor the laborious investigations of the Germans; they confess that the doctrines taught are forcible, and that their modes of investigation are

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wonderfully accurate and ingenious, but yet they regard these selections as being useless to the great mass of our readers, either because they are not directly practical in their character, or too abstruse and complicated to be appreciated.

We will endeavor in a few words to answer these objections, and will premise by enquiring, why do our readers take a medical journal? What is, or rather, what ought to be the object of all such publications? We regard them as intended to increase our scientific knowledge, to enlarge the horizon which oftentimes binds us down to bigoted dogmas and illiberal views; to cultivate a devotion for the noble profession which we call ours, and make us admire it more and love it better; to incite ambition and awake a healthful spirit of emulation; and lastly, to gather in from all directions the many valuable facts and unrecorded experiences of our brethren.

With these desires we have ever labored to make this journal as interesting and original as our opportunities would permit. Whilst every contribution from the profession of the state has been gladly received, and indeed urgently sought after, we have devoted much of our space to the discussion of novel and interesting articles gathered from abroad. Many a bold innovation and many an untenable theory has found its way to our columns; but the tendency of all was to make us think, to rouse us from the apathy and idleness, too often the accompaniments of ignorance and self-conceit, and excite in us a spirit of enquiry, which would lead us on into new and hitherto untrodden paths.

With this object, we were among the first to dig freely into the mine of French medical literature, confessedly the most teeming with riches, and the most remunerative to those who choose to labor amidst its wealth of material. Whilst we acknowledge how sound and reliable are our English brethren—and none will dispute the sterling worth of their contributions to science—all must admit that in the investigation of the great sciences of physiology and patho-

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logy, in the dexterity and appositeness of her experimentation and in her improvements in therapeutics, the school of Paris has stood of late years unrivaled.

We cannot doubt that those who have read the many brilliant essays and monographs which have from time to time appeared in our columns, will acknowledge that they have thus received both instruction and profit. The great experience of Velpeau and Malgaigne in surgery, of Trousseau and Rilliet in medicine, must be instructive to those not already familiar with their works. The admirable description of the anatomy, pathology and symptoms of stricture, and the valuable series of articles begun in the present number, on the treatment of aneurism, introduce our readers to new views and new experiences, and they are enabled to form their opinions not from contracted and limited sources, but with all the literature of the day spread before them.

We will conclude these remarks by expressing an earnest desire to combine with the finished essays and scientific lectures of the European physicians, the practical deductions and valuable observations of our own brethren. Our crowded columns give evidence that we have not appealed to them in vain; and we have reason to hope that the Virginia profession are not unwilling to aid us in our efforts to improve the character and usefulness of our work.

American Medical Association.

The ninth annual meeting of the American medical association will be held in the city of Detroit, Mich. on Tuesday, May 6th, 1856.

The secretaries of all societies, and other bodies entitled to representation in the association, are requested to forward to the undersigned correct lists of their respective delegations, *as soon as they may be appointed;* and it is *earnestly* desired by the committee of arrangements that the appointments be made at as early a period as possible.

The following extracts are from article second of the constitution:

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"Each local society shall have the privilege of sending to the association one delegate for every ten of its regular resident members, and one for every additional fraction of more than half this number.

"The faculty of every regularly constituted medical college or chartered school of medicine, shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital, containing a hundred patients or more, shall have the privilege of sending two delegates; and every other permanently organized medical institution, of good standing, shall have the privilege of sending one delegate.

"Delegates representing the medical staff of the United States army and navy shall be appointed by the chiefs of the army and navy medical bureau. The number of delegates so appointed shall be four from the army medical officers, and an equal number from the navy medical officers."

The latter clause, in relation to delegates from the army and navy, was adopted as an amendment to the constitution, at the meeting of the association held in New York in May 1853.

WILLIAM BRODIE, M. D. *Detroit, Mich.*
One of the Secretaries.

Special Notice.

☞ The delegates to the American medical association are requested to report themselves, on their arrival at Detroit, in Fireman's hall, corner of Jefferson avenue and Randolph street, where the annual meeting will be held; or to the secretary, Dr. Wm. Brodie, Masonic hall.

The Medical Department of the French Army.

UNTIL very lately the medical officers of the French army were divided into physicians, surgeons and apothecaries. By the ordonnance of the 23d March 1852, which introduced considerable modifications into the department, and placed it on its present footing, the distinction between physicians and surgeons was abolished, and two branches only are now recognized, medical officers (médecins) and apothecaries. The following are the various grades and numbers of each borne on the strength of the army at its peace establishment:

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	Médecins.	Pharmaciens.
Inspecteurs,	7	1
Principaux, 1re Classe,	40	5
" 2de "	40	5
Majors, 1re Classe,	100	15
" 2de "	220	30
Aides-Majors, 1re Classe,	340	45
" 2de "	340	45

The corps of medical officers is placed under the authority of the minister of war, who appoints annually a council of health consisting of four médecins-inspecteurs and one pharmacien-inspecteur, with a médecin principal or major as their secretary, to assist him with their advice on all professional questions.

When the number of officers on the strength of the establishment is inadequate to the requirements of the service, "auxiliary" medical officers from civil life may be appointed by warrant from the minister of war, or may be employed by the officers of the "intendance militaire."*

They are not, however, incorporated with the regular staff, but are classed after that branch of the department to which they belong.

The aides-majors are appointed from the students of the special school of military medicine and from the "auxiliary" officers. The former must have studied the regulated time in the school—must have passed the required examinations—and must possess the university degree of doctor, if appointed to the medical, or of master in pharmacy, if to the pharmaceutical branch of the service. The "auxiliaries" are eligible for permanent appointments after having served two years and made one campaign, but they too must possess the degrees. Vacancies are filled up in the proportion of three-fourths from the school and one-fourth from the "auxiliaries."

The following statement shows the period an officer must serve in each grade before being eligible for promotion, and the principle on which the promotions are made:

Médecin et Pharmacien.	Must have served in his present rank at least	Promotion made by	
		Seniority.	Selection.
Aide-Major, 2de Classe,	2 years.		
" 1re "	2 "	Two-thirds.	One-third.
Major, 2de Classe,	4 "	"	"
" 1re "	3 "	One-half.	One-half.
Principal, 2de Classe,	2 "	None.	All.
" 1re "	3 "	None.	All.
Inspecteur,	–	None.	All.

* The "intendance militaire" is a corps to which there is nothing analogous in our army. It may be described as the civil department of the staff; there is a branch of it charged with the administration of the hospitals in everything except the professional treatment. The sous-intendant appears to have duties to perform similar to those of a purveyor united to the obsolete appointment of commandant of the hospital in our service.

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These periods, however, may be reduced by one-half in time of war, and may be dispensed with altogether if the exigencies of the service require it, or in case of an act of devotion and courage duly authenticated and published in the orders of the army or of the division.

The pay and allowance of the officers of the department are shown in the following table:

Médecin ou Pharmacien.	Full pay.		Pay when not employed.		Additions to pay.	
	Per annum.	Extra when doing duty in Paris.	From reduction, suppression of employment, return from captivity, temporary infirmities.	From withdrawal or suspension of employment.	Allowance in lieu of Lodging.	Allowance in lieu of Furniture.
	£	£ s. d.	£ s. d.	£ s.	£ s.	£ s.
Inspecteur,	352	–	176 0 0	140 16	48 0	16 0
Principal, 1re Classe,	200	40 0 0	100 0 0	80 0	38 8	12 16
2de "	180	36 0 0	90 0 0	72 0	33 12	11 4
Major, 1re Classe,	140	28 0 0	70 0 0	56 0	28 16	9 12
2de "	112	28 0 0	56 0 0	44 16	14 8	7 4
Aide-Major, 1re Classe,	90	26 0 0	54 0 0	36 0	9 12	4 16
2de "	74	24 13 4	44 8 0	28 12	9 12	4 16

In addition to the above pay, the inspecteurs receive £88 per annum when the army is on the footing of war. When they are on the unemployed list, but waiting for employment, their pay is £200 a year. An addition of one-third is made to the lodging and furniture allowances of all officers doing duty in Paris.

The medical officers are entitled to retired pay under the same regulations as the other officers of the army, but are permitted to count as service five years spent in prliminary studies before their appointment as aides-majors. The minimum rate of pension is granted after thirty years' full pay, and there is an annual increase at a fixed rate up to fifty years' service, when the maximum is attained. There are certain circumstances which entitle an officer to count additional time at the rate of two years, and sometimes more, for every one of actual service. The chief of these are, when the army is on the war establishment; when the officer is serving with a *corps d'armée* occupying a foreign country; or serving on board ship in a maritime war; or serving out of Europe in time of peace.

The age at which medical officers are entitled to claim their retirement, and the rate of retired pay in each rank, are shown in the following table:

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Médecin ou Pharmacien.	Maximum.	Minimum.	Age at which entitled to claim retirement.
	£ s. d.	£ s. d.	
Inspecteur,	160 0 0	120 0 0	64
Principal, 1re Classe,	120 0 0	96 0 0	60
2de "	96 0 0	72 0 0	
Major, 1re Classe,	80 0 0	60 0 0	58
2de "	64 0 0	48 0 0	56
Aide-Major, 1re Classe,	48 0 0	32 0 0	50
2de "	40 0 0	24 0 0	

Medical Staff of the French Navy.

According to the report of the minister of marine on the reorganization of the health-service of the French fleet, the medical staff of the imperial navy is composed as follows: One inspector-general, with the assimilated grade of vice-admiral; 3 directors, rank, commissary-general; 3 physicians or surgeons-in-chief, and 3 head-pharmaciens, with the rank of captain of ship of the line; 4 adjunct-physicians-in-chief, 5 adjunct-surgeons-in-chief, and 3 adjunct-pharmaciens-in-chief, with the rank of frigate-captains; 9 professors, rank of chief of battalion; 25 principal surgeons, and 70 surgeons of the first class, with the rank of commissary; 9 pharmaciens of the first class, 180 surgeons of the second class, with the rank of lieutenant of ship; 14 pharmaciens of the second class, 40 surgeons of the third class, and 20 pharmaciens of the third class, with the rank of ensign. "Military honors shall be paid to the medical officers according to their assimilated grade." *Moniteur, Av.* 1854.

In the *Bulletin de Thérapeutique*, the duties of the several classes of medical officers are defined. Each *director* has charge of a division of the medical force; he corresponds with the inspector-general and maritime prefect, and furnishes confidential notes in regard to the conduct of those under his orders; he presides at *concours* and boards of health; every year he makes a detailed report of his service, and proposes improvements and reforms. Whenever a surgeon of the first class is to be promoted by *choice* to the grade of *principal-surgeon*, the director convenes the board of health of his division, in secret session; a report of this session, with confidential advices from the director, is transmitted to the minister. The *principal surgeons* are employed, according to the exigencies of the service, ashore or afloat. They go to sea at the order

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of the minister of marine, assuming the functions of surgeon-major of division (or fleet-surgeon;) and, in this capacity, belong to the general staff. The *directors* are *chosen;* no surgeon is eligible to this post unless he has served for two years as head-surgeon or head-physician at Brest, Toulon, or Rochefort. The *principal surgeons* obtain their grade partly by seniority, partly by choice; no one can be a candidate for this rank unless he has served eight years as surgeon of the first class. The pay, exclusive of rations, is as follows, if we rate five francs to the dollar: inspector-general, $2,000; directors, $1,400; professors, principal surgeons, $600; surgeons of the first class, $480; surgeons of the second class, $360; surgeons of the third class, $240.

The United States Naval Medical Board.

The naval medical board, which convened at Philadelphia on the 1st of March last, have concluded their examinations, and reported the following assistant surgeons in the navy for promotion:

Wm. F. Carrington—to rank next after Passed Assistant Surgeon John Ward.

James Studdards—to rank next after Passed Assistant Surgeon Francis M. Gunnell.

James F. Heustis—to rank next after Passed Assistant Surgeon Edward Shippen.

Arthur Lynah—to rank next after Passed Assistant Surgeon James F. Heustis.

Samuel F. Coues—to rank next after Passed Assistant Surgeon Arthur Lynah.

Charles F. Fahs—to rank next after Passed Assistant Surgeon Samuel F. Coues.

Jacob S. Dungan—to rank next after Passed Assistant Surgeon Jacob S. Dungan.

George Peck—to rank next after Passed Assistant Surgeon Charles F. Fahs.

Jenks Harris Otis—to rank next after Passed Assistant Surgeon Geo. Peck.

Fred. Horner, jr.—to rank next after Passed Assistant Surgeon Jenks Harris Otis.

The following named gentlemen, arranged in the order of their relative merit, were found qualified for the office of Assistant Surgeon of the Navy:

No. 1. H. A. F. Washington of Virginia.

No. 2. Richard C. Dean of Pennsylvania.

No. 3. H. Lawrence Sheldon of Connecticut.

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No. 4. Philip S. Wales of Maryland.
No. 5. Albert C. Gorgas of Pennsylvania.
No. 6. Alexander M. Vedder of New York.
No. 7. Delavan Bloodgood of New York.

Life Insurance.

EVERY traveler who visits our country remarks upon the utter recklessness with which life is regarded by the great mass of the people, who, whilst they are proverbially shrewd and careful in taking care of their property and interests, yet do not hesitate to risk their lives upon the most unnecessary and trivial occasions.

This fact is exemplified in the neglect with which we regard the system of effecting policies of insurance on lives, which, though of modern origin, has already effected an amount of good hardly measurable to the eye of the casual observer. Whilst the system of life insurance is encouraged by all classes of the community in England, Scotland and on the continent of Europe, with us, as yet, but few have availed themselves of this wise precaution; and whilst every prudent man insures his house, his furniture, or his ship and cargo, which *may* be lost, he neglects to insure his life, which he knows *must* be taken away.

These remarks have been suggested by a table in a late number of the Edinburgh Medical Journal, which shows that in the year 1854 more than *nine thousand* persons were insured in the small kingdom of Scotland, and that the emoluments received by the profession, for their services as medical examiners, amounted to $35,000. This evidence of the well known provident and careful habits of that people should be a lesson to us, and ought to call our attention particularly to this subject.

An interesting pamphlet, lately received from the New York Life Insurance Company, illustrates the profitable character of such investments when effected in sound and reliable institutions. These calculations show that the premium annually paid on the life policy can be made to produce an eventual profit to the person insured, vastly greater than by any other mode of investment, and that the best of all savings banks is a life insurance.

To professional men especially these arguments particularly apply. Without the possession of realized capital, many lawyers and physicians are in the receipt of an income ample enough to permit them to devote a few hundred dollars yearly to this purpose, and thus, by the exercise of a moderate economy during life, their wives and children are effectually guarded against the poverty and wretchedness which, alas! in so many instances is now their fate.

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VARIETIES.

The Death Tic—A Chapter on Medical Ethics.

We were strolling down Regent street the other afternoon, gravely meditating, as usual, on the state of the medical profession, and the growing influence and utility of the Medical Times and Gazette, when we were joined by an old school-fellow, a partner in an old-standing legal firm, who was sauntering home from Lincoln's-inn, to his house in the regions once called Tyburnian, but now Westbournian. Such magic is there in the change of one syllable! We had not seen our friend for three or four years, and the last we heard of him was that he was in a very delicate state of health. So we congratulated him on looking much stouter and more robust than of old.

"Ah, my dear fellow," he replied, "thank heaven, I feel very well now; quite alive, and up to every thing. But 'twas quite another story four years ago. I was seedy enough then. I am well enough now, though, no thanks to the doctors. Ever since that time I have been at war with your learned profession, I can tell you."

"But, why?" we replied; "you were ill, and are well; what wrong have the doctors done you? I thought you were going to give us some credit for your recovery."

"Not a bit. But I'll tell you the case, and leave you to judge for yourself. I had been dreadfully worried; things did not go right at the office; money was uncommonly hard. I could not venture to tell my grievances even to my wife, and I got little sleep; took little breakfast, no luncheon, except bitter beer; then, at dinner, I ate fast and furious; no real appetite; but I tried to get it over, as a disagreeable duty, and then took a good deal of wine. I see all this clearly now, though I could not see the harm of it at the time. Well, while in this condition, one evening I had an attack of cold shivers; next day, up I would get, and insist on going out; but, the following morning found old Dulph, the family apothecary, at my bedside. Kind old man! He uses the word *we* for every body. 'I see we are feverish, my dear sir,' he said; 'we must keep abed; we must send you some draughts, one every four hours; we must promote gentle perspirations, and so on.' In fact, to make a long story short, I was in for a bad influenza, and I was not out of it for a long, long time. Week after week, I was as weak as a rat, coughing incessantly. My friends began to ask if Mr. Dulph had applied the stethoscope; but we could get nothing very satisfactory out of him. He said that we had a great deal of weakness to contend with, but the tonic draughts, and change of air, would set me to rights, and yet that it would be a work of time.

"So, while things were in this plight, and I could plainly see that my friends felt very shaky about me, and one was advising me to call in Doctor This, and another Doctor That, at last, as a satisfaction to my family, and also to see if I could get some relief, I went one morning with my wife, without saying a word to old Dulph, to consult the great Dr. Cæruleus Mortisage, physician to St. Lazarus' infirmary for diseases of the windpipe, fellow of a dozen societies, professor of mortal pathology, or something of the sort, at the collegiate school of medicine, and altogether a medical gun of the greatest possible calibre. Well, sir, I was stripped, and weighed, and measured, and tapped, and rapped, till my ribs ached; I was made to gasp, and breathe, and cough, and count; every inch of me was explored; but I could plainly see that the more I was explored, the worse was the doctor's opinion of my case. At last, when he had finished his investigation, we asked him anxiously what he thought. And I scarcely can

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fancy a poor wretch at the Old Bailey looking more anxiously in the foreman's face when he comes in to deliver the verdict of the jury, than I did at the face of Dr. Cæruleus Mortisage, in order to anticipate what *his* verdict would be. As for my poor wife, she trembled so, that I thought she would have dropped off her chair.

"However, after a short preamble, the opinion was delivered, calmly, and not unkindly; but it was very positive, and unfavorable enough in all conscience. He told me a good deal which I could not understand, about congestion, or consolidation, or something of that sort, but said it was his duty to tell me that he considered my case a bad one; he believed I should not be able to go on with my usual pursuits, and hinted at the expediency of parting with my share in the business, and retiring to Bournemouth or Hastings. He spoke long and kindly, but all to the same purpose, and took no great pains to conceal the fact that he thought me a dead attorney.

"Just as we were going (for I had paid him his fee) it struck me that something was missing, I could scarcely recollect what, so stunned was I at the sentence that had been passed on me, till my wife said, 'Are you not going to give us any prescription, Dr. Mortisage?'

"The question seemed to strike him all of a heap, for he had evidently forgotten this part of his office; but he took up his pen and wrote something, which he gave me, and we left.

"So soon as we got into our carriage, and I could collect my thoughts a little, my anger knew no bounds. Here is this rascal, I said, pronouncing sentence of death on me, and scarcely thinking it worth his while to write a prescription; what good can such a man's treatment do me? He begins with the notion that treatment is of no use, and when that is the case, what good is he likely to do? So I tore the prescription into fifty pieces, and threw them out of the window. If I am to die, I said, be it so; but I'll have a good stand-up fight for life first of all. So, back I went to old Dulph, who gave me lots of tonic draughts, then I went to stay with my wife's aunt in the country, an uncommon old lady for feeding one up, and then I found out that things at the office were not so bad as I feared; in fact, if I had fretted less they never would have seemed so bad, nor have been so bad, and every thing went on very well without me, till after about six months of care, and nursing, and change, I got better, and here you see I am, not a bit like a dying man. The next time I am ill, depend on it I won't go to any of these ominous death-tic fellows; if I had not been a strong-minded man, I should have died of fright. When I am ill next I'll choose a man who has a heart under his waistcoat; one who don't look upon his patient merely as a mass of decayed lungs or arteries or what not, but who will put a little heart and soul into the work of cure, who will fan one's hopes and desires of life, and so feed the flame, instead of putting a wet blanket upon it, and seeming as if his whole soul were bent upon a post-mortem examination. For my part, I never meet Mortisage in society without a kind of awkwardness; I feel somehow as if my being alive and walking about was a sort of standing imputation on his skill in diagnosis; this feeling quite haunts me, and I sometimes wonder whether a physician could not bring an action against a man for obstinately, pertinaciously, and offensively living on from year to year in spite of the clearest and most scientific evidence that he ought to have died long ago."

Thus far our loquacious and iracund attorney had spoken, when we parted with mutual salutations, he to his dinner, and we to ours.

In spite of the apparent exaggeration of some of his statements, it occurred to us that they might usefully furnish food for reflection to our readers. Certain it is, that the profession is quite conscious of the existence of a class of practitioners—*and society is not quite ignorant of it either*—who are too apt to look on human beings as mere objects; who, in cultivating the art of distinguishing diseases with the greatest possible accuracy, are not equally studious of the means of curing them; who regard physical alterations of structure to the neglect of that vital force which overrules the whole machine; and who, above all, practice in a hard, cold spirit, without exhibiting anything positively wrong, or inhuman, or unkind, but yet without the sympathy, the heartiness, as it were,

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requisite for genial and successful influence upon man in his entire nature. This is the fault which leads them to be ready rather to foretell impending mischief, than to prevent it; and it is most curious to observe how prevalent the custom of giving hopeless but wrong prognoses has been, in the case of diseases of these very organs, the accurate diagnosis of which is among the greatest triumphs of modern medicine.

There are few of us, perhaps, who cannot call to mind some condemned case of phthisis, or of disease of the heart, in which the sufferer has continued to live out his time, in defiance of mortal diagnosis. The moral we would draw, at present, is the necessity of caution in all cases, in giving opinions which exclude all hopes of recovery, and of never neglecting, in an apparently incurable malady, such treatment as would be most likely to preserve life, if the case admitted of recovery.—*Med. Times & Gaz.*

Foreign Items.

A CURIOUS EVENT occurred a few days ago in the University of Liepsic. Dr. Reclam, professor of legal medicine, was lecturing on nicotine; and to show the deadly effects of the poison, he administered a large dose of it to a big dog. The animal, which was lying on its back, was immediately seized with convulsions, and ejecting a considerable portion of the poison with violence, it struck the professor on the face, and some of it entered his mouth. He was immediately taken with the symptoms of poisoning, but antidotes were promptly applied, and he was soon placed out of danger.

The recompenses of the French Académie des Sciences have been awarded this year:

1st. To Dr. Hannover, of Copenhagen, for his important researches on the anatomy, physiology and pathology of the eye—1,500 francs.

2nd. To Professor Lehmann, of Leipzig, for his capital work on physiological chemistry—1,500 francs.

3rd. To Dr. Cazeaux, for a paper on chloro-anæmia in pregnant women.

4th. To Dr. Tardieu, for his great work, entitled "Dictionnaire d'Hygiène Publique, et de Salubrité," in three vols.—1,000 francs.

5th. To Dr. Beau, for a paper on the physiology and pathology of the spleno-hepatic apparatus—1,500 francs.

6th. To Dr. Foissac, for his book, entitled "De la Météorologie dans ses Rapports avec la Science de l'Homme, et principalement avec la Médecine et l'Hygiène Publique"—1,000 francs.

7th. To Dr. Béraud, for his researches on the anatomy and pathology of the lachrymal organs—1,500 francs.

8th. To M. Dareste, for his researches on the circumvolutions of the brain—1,000 francs.

9th. To M. Bouquet, for a paper on the chemical composition of the mineral waters of Vichy—1,500 francs.

10th. To Dr. Corvisart, for his researches on the therapeutical influence of pepsin—1,500 francs.

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Among the essays rewarded by the academy, that of Dr. Cazeaux is one of the most important. This distinguished accoucheur proves that true plethora is very rare in pregnant women, and that a state of chloro-anæmia is frequently mistaken for it. All the symptoms of chlorosis (*bruit de souffle* in the carotids, disturbances in digestion and in the nervous system, and diminution of the globules of the blood) have been found by Dr. Cazeaux in women who had been considered as plethoric; besides, instead of bleeding them, he has given iron and meat, and has cured them.

The academy has consented to offer a prize which will be the object of many researches, on account of its pecuniary value, which is 100,000f. (£ 4,000.) I am afraid the money will be long in the hands of the academy, as the question is not likely to be solved soon. The will of M. Bréant, the generous man, who has left this large sum to the academy, expresses his object in the following sentence: "I establish and give to the Institute of France, a prize of 100,000f. to be awarded to the person who will find a successful treatment of Asiatic cholera, or discover the cause of this disease."

THE CONTUMACIOUS STUDENTS OF THE SORBONNE.—The students arrested some time since on account of the "row" at M. Nisard's lecture at the Sorbonne, have been set at liberty, and are to be sent to their homes. They are mostly from the provinces. They can continue their studies in medicine or law, if they think proper, in the departments, but are not to return to Paris, and are to give notice to the authorities of the school. The police agents in plain clothes who were stationed in the hall marked with chalk the backs of the most violent, in order to identify them when they got into the streets. The students discovered the artifice, and without saying any thing, marked with chalk also the backs of the agents themselves, so that several of the latter were arrested by the sergens de ville, and in spite of their protestations lodged in prison, and kept there for a whole day and night.

MYCOLOGY.—Mr. Ravenel has just issued the fourth fasciculus of his work on the fungi of Carolina. A large number of new species are described. We sincerely trust that the esteemed author's researches in this difficult and almost unexplored field will be appreciated and adequately encouraged.

NECROLOGY.

BARTON.—W. P. C. Barton, senior surgeon of the U. S. Navy, died at Philadelphia on the 27th of March. Dr. Barton had belonged to the medical staff of the navy for forty-seven years, having entered the service in 1809. He counted twenty-five years of shore duty, and three and a half years of sea service, having been last at sea in 1830.

DR. THOMAS J. BOYD, the oldest practitioner of medicine in the city of New York. He was in his 84th year, and had practiced medicine *sixty-three years.* He had always enjoyed a large and lucrative practice, and indeed may be said

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to have "died in harness," as within a week of his death he was seen tottering on his way to visit a patient, supported by a dutiful daughter, who was the companion of his later years.

JOHNSTON.—The well known agricultural chemist, Dr. Robert Johnston, author of Chemistry of Common Life, died at Durham, England, after a short illness.

MORAWECK.—Herr Moraweck, professor of surgery at the school of Wurtzbourg, recently died of pneumonia.

LATTIMORE.—Dr. W. P. Lattimore, a young physician of promise, the translator of one edition of Ricord's letters, died in New York, January 5th, aged 29 years.

COPELAND.—Thomas Copeland, an eminent English surgeon, died recently at Brighton, at the age of 74. He left a large property, and liberal bequests. Among others was a bequest of $25,000 to the society for the relief of widows and ophans of medical men in London.

BARBIER.—Dr. J. B. G. Barbier, physician-in-chief to the hospital of Amiens, professor of pathology in the preparatory school of medicine of that city, member of many scientific societies, an excellent botanist and a voluminous writer, died on the 21st of November last, at the age of 79.

HAINDL.—The Gazette Medicale of December 1st states that Dr. Haindl, director of the great hospital at Vienna, recently died of cholera, at the age of 52. The eminent post he occupied is temporarily filled by Dr. Prinz.

HARRIS.—Dr. Thaddeus Harris, librarian of Harvard university, died at Cambridge on the 16th of January, at the age of 60 years. Dr. Harris practiced medicine with reputation in his native town of Dorchester until 1831, when he was appointed librarian to the college at Cambridge. From this date until his death, in addition to the faithful performance of the duties of his office, he worked hard in different branches of natural history. He was pronounced by Agassiz to be "decidedly the best entomologist in the world."

BALLINGALL.—Sir George Ballingall, regius professor of military surgery at Edinburgh, died on the 4th of December last.

CHAMBERS.—Wm. Fred. Chambers, M. D. K. C. H. F. R. S. formerly physician to St. George's hospital, to William IV and queen Adelaide, to Victoria, etc. who "led the town" from 1836 to 1848, died on the 16th of December last, at the age of 70 years.

CLOQUET.—Dr. Ernest Cloquet, nephew of the brothers Hippolyte and Jules Cloquet, the former the most famous of anatomical lecturers, the latter a brilliant surgeon, died recently in Persia, where he resided as physician to the Shah. His death is said to have been caused by an over dose of cantharides taken by mistake. We are indebted to this physician for interesting observations on oriental diseases, and on the state of medicine in the east.

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VIRGINIA MEDICAL JOURNAL.

THE STETHOSCOPE

AND

VIRGINIA MEDICAL AND SURGICAL JOURNAL

COMBINED.

JUNE 1856.

ART. I.—*Surgical Reports.* By CHARLES BELL GIBSON, M. D. Professor of Surgery in the Medical College of Virginia. (Continued from p. 188.)

Strangulated Hernia.

IN eleven cases of operation for strangulated hernia, I have had three which terminated fatally, and one in which artificial anus occurred, and was subsequently cured. Those cases in which the operation failed to save life are reported, because of the instruction derivable from their history. The successful cases present nothing worthy of especial remark, not differing from a thousand others already published. The solitary case of the establishment of artificial anus will be found, it is hoped, interesting and instructive.

CASE I.—Francis Buck, teacher of music, had for years irreducible inguinal hernia of the right side. On the 20th June 1848 he consulted Drs. Hancock and Bohannan about a pain in the abdomen and constipation. The usual remedies were administered, but the symptoms increased in intensity. Concealing from his physicians, through a false sense of delicacy, the existence of a hernia, they only be-

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came aware of it on the morning of the 22nd, after four days of excessive suffering. The occurrence of stercoraceous vomiting betrayed the nature of the cause of the constipation. Efforts at taxis were immediately and most thoroughly made, and proving unavailing, my services were asked in the performance of the operation. Mr. Buck's condition was most unpromising at 7 o'clock on the 22nd, when the operation was performed. The surface was cool and moist; tongue brown; pulse rapid and small; abdomen tympanitic, and the hernial tumor livid. This array of symptoms foreshadowed the condition of things in the hernial sac. On being opened, the tumor was found to consist of intestine, omentum and the right testis, which had never descended, and which consequently explained the phenomenon of the hernial tumor not descending as it usually does into the scrotum. The intestine was so far advanced in inflammation as to leave but little hope that sphacelation could be avoided by the division of the stricture, still it was deemed possible, and the contents were therefore returned to the abdomen, after dividing the stricture at the external and internal rings.

Every effort was made after the operation to sustain the patient, but he continued to sink, and died before night.

In this case we find a remarkable instance of the pertinacity with which a false sense of delicacy will induce a patient to conceal what he undoubtedly supposed to be a personal defect. This man had apparently but one testis. He was ashamed to have the fact known; and therefore, at the risk of his life, denied having a tumor of any kind in the abdominal region, when closely interrogated on the subject by his physicians. Perhaps it is not too much to say that his life was forfeited to his unhappy error.

Another point of interest in this case is the finding of the testis in the sac. The tunica vaginalis testis is originally a process of peritoneum, which the testis in its descent through the rings to the scrotum, pushes before it. This tunic is found closed at birth generally, and is thus sepa-

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rated from the rest of the peritoneum. In this case there being no descent of the testis, there was of course no tunica vaginalis, and the testis, intestine and omentum occupied one common peritoneal sac.

Case II.—Winnie, servant, belonging to Mr. J. M*** had been complaining for several days of rheumatic pains, and had taken several drachm doses of the wine of colchicum. These had produced severe vomiting and purging, and the family physician (Dr. Deane) was then called in on the 20th November.

On the 21st the purging ceased, but the vomiting continued. On the 22nd severe pain was felt over the abdomen, and particularly in an old *irreducible* femoral hernia of the left side; fever existed, and the vomiting continued, and became stercoraceous; the bowels were now obstinately constipated, resisting castor oil, turpentine and ninety grains of calomel given in the space of nine hours.

Although the earlier symptoms were clearly due to the large doses of colchicum, it became apparent on the 23rd that the old irreducible hernia had become strangulated, and I was requested to operate. A stricture was found at the saphenic opening, and another at the crural ring. The contents of the sac, intestine only, being in good condition, were returned to the abdomen. At 10 o'clock at night it was apparent this woman must die. Cold surface, flickering pulse, rapid respiration and confused intelligence, all marking the downward progress of the case. She died early in the morning of the 24th.

Late on the 24th an autopsy was made. The intestine, which had been the seat of strangulation, was inflamed for the space of an inch, but no approach to sphacelation existed. The rest of the exterior of the intestinal tube was uninflamed, but the mucous coat throughout the small intestines was softened and easily detached. The stomach was intensely inflamed. Its mucous coat was completely softened, and was readily scraped off with the knife handle.

I have no doubt that the fatal termination of this case was

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due to the poisonous effect of the colchicum, for the post-mortem examination revealed a condition of stomach and bowels not to be explained upon other reasoning. It is more than probable also that the excessive purgative action of the colchicum caused a hernia, which had been for years harmless, though irreducible, to become strangulated; and thus developing another serious source of danger, attracted the attention of the medical attendant to the relief of what some persons might have considered as affording ample explanation of all the symptoms. The entire absence of general peritonitis and the slight degree of inflammation in the tissue of the incarcerated intestine, prove that the fatal result was not due to the hernia; and while we could not disregard the appeals which the symptoms of strangulation made for the performance of the operation, we are satisfied that we were right in supposing there existed a condition in the stomach defying all surgery.

Case III.—Mrs. S*** of Louisa county, aged 60, a very large woman, made a rough journey to Goochland court-house about the 14th of September last. The jarring and jolting of the journey affected painfully a large tumor in the umbilical region, which Mrs. S. had carried many years. On the 16th, Dr. Harris of Goochland and Dr. Michaux of Powhatan saw the lady, and recognized the tumor as umbilical hernia. Her symptoms then were, pain in the tumor and over the abdomen generally, vomiting and constipation. Efforts were made to reduce this large tumor, but without success; and fear existing that strangulation had taken place, induced these gentlemen to request my advice. I examined Mrs. S. at 4 o'clock P. M. on the 17th, and concurring in opinion with my friends, advised immediate operation. It seems, however, that the poor lady had been once before in a situation very much resembling her present condition, and had been entirely relieved by taxis employed by her family physician, and she was extremely anxious, before any operation should be performed, that he should see her and endeavor to reduce the tumor. Her wishes were unfortunately respected,

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and Dr. Pendleton was sent for, and twenty hours elapsed before the messenger returned, and then without him.

Much valuable time had been lost, and the patient's condition evidently showed it, for by the time the operation was in progress she manifested unmistakable signs that mortification had occurred. The pulse, skin, countenance and tumor all showed it, and added to these, was that surest of all signs, cessation of pain. Still, however, there was a possible chance, and the operation went on, (September 18, 4 P. M.) disclosing a mass of intestine and omentum protruded through the umbilical ring, not only sphacelated but broken down, with the fluid contents of the intestine escaping into the sac. Death was of course inevitable; but still we stitched the living intestine to the edges of the wound, and partially closing the wound with a few interrupted sutures, we took sad leave of the patient. She died in 24 hours.

This case might have been saved, it is probable, if an early operation had been performed; but this was not possible under the peculiar circumstances.

Case iv.—A. P*** aged 25, had had scrotal hernia several years. Being intemperate and careless, he, from neglecting to wear his truss, had been threatened several times with strangulated hernia, but fortunately taxis had always, up to this time, been successful.

On the 30th January I was requested by Dr. Waring to operate upon this patient, whose hernia had been irreducible five days. Assisted by Dr. Waring and my friend Dr. Wellford, I exposed the sac, opened it, and found a mass of omentum and intestine presenting a suspicious appearance. Mortification was not, however, present, only a little threatening, and we determined to risk returning the intestine to the abdomen, with the hope that the constriction being removed from the intestine, the circulation in it would once more become natural.

Some four hours after the operation Mr. P. had a free evacuation from the bowels. The tympanitic condition, which was intense before the operation, was greatly re-

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lieved. Sleep was obtained, and the patient had every prospect of recovery. Every day showed some improvement; the evacuations were regular and spontaneous; appetite returned; sleep was natural and profound. On the eleventh day after the operation, (10th February,) in dressing the wound, (which by the way was rapidly closing,) we noticed the discharge of some dark, thin matter, which proved to be fœcal. In the course of the day a considerable quantity passed out, creating inflammation in the wound, and causing to be partially undone the repair which had already taken place. It was impossible, by the most careful examination, to see the opening in the intestine from which the discharge came, for the parts were matted together by effused fibrin; and although the edges of the wound, now from irritation everted and swollen, were some three inches in length, yet the opening conducting towards the intestine was not more than 3 lines in diameter. It was evident, however, that but a small portion of the intestine had sloughed, perhaps only a point in the immediate line, where the constriction was experienced, and we had no fear of fœcal extravasation, as ample time had elapsed since the operation for those protective fibrinous barriers to be formed with which nature so admirably protects important tissues under such circumstances.

For two days the discharge through the artificial anus was very free, and no evacuations occurred by the natural route. But on the third day a portion of fœcal matter passed naturally, and the next day a little more, and so on, each day the opening at the groin becoming smaller and smaller, and the discharge through it less and less, until finally my note of the case, taken 18th April, records "complete closure of the fistulous opening, with perfect re-establishment of the normal route."

During all this time very great attention was paid to the diet and position of the patient, and to the enforcement of cleanliness about the artificial anus, which was also occasionally touched with the nitrate of silver. No pressure

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was made upon the orifice by pad, or truss, or otherwise, reliance being placed chiefly upon the principle that a minute and unnatural channel cannot contend with the natural and larger passage, and that each day's diminished use of the new route would secure its certain and permanent contraction until it should be completely closed.

There are three points of interest in this case:

The first is, that after five days of strangulation, the intestine was found in a condition to admit of being returned to the abdomen.

Another point is, that the release from constriction allowed such invigoration of the circulation in several inches of intestine as to rescue it from impending gangrene, with the exception of probably a very small point.

Another is the admirable mode which nature employs to shut out the peritoneum, by the effusion of fibrine, from the tract leading from the intestine to the surface, thus securing the patient from the danger of fœcal extravasation upon the peritoneum, and protecting his life, whatever the inconvenience might chance to be.

And lastly, one more point of interest is, the happy termination, by nature's surgery, comparatively unassisted, of an affection so loathsome, that it may be a question whether death may not be preferable to life under its continuance.

ART. II.—*A few Observations on the Attributes of the Impregnated Germ.* By R. H. TATUM, M. D. of Chesterfield County, Va.

IT is not our design to present the history of generation—the progress or means by which the spermatozoa reaches the ovum and impregnates the ovule. That the germ becomes fecundated and is the starting point in the production of all animal being, is sufficient for our purpose. Some misapprehension in relation to the characteristics of the *embryo* seems to exist. The legislators of our own and other countries

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appear not to have followed the light which the progress of physiological science has thrown upon this subject. So far as I have been able to inform myself, but few states have enacted statutes even in relation to abortion. New York, Missouri and one or two other states have statutes declaring it punishable by fine and imprisonment. Others have loitered by the way, content with the statutes on premature labor.

We state this point, and desire for it some consideration—that the impregnated germ, whether as *embryo* or fœtus, is a *distinct, living, immortal being*, and should be so considered by our laws. I may be asked, how do I know it to be a sentient being, having all the attributes of immortality? and as a consequence demanding the protecting care of the law. What visible or audible evidence have you that makes it entitled, *after* quickening, to its present consideration, in the statutes, that it does not present at any period of gestation? Can that be sustained, and not the other? Now, all physiologists of the present time concur in the opinion that all animals have their origin in a germ. It is the first step in the process of development, and is the result of copulation between the two sexes. It is likewise known that the *germ* has the power of appropriating and assimilating all nutriment necessary for its development. That this germ begets other germs, and so on till the maturity of the being. All beings have their characteristics and a *certain* and *unchanging* process of development. The ability to attain such a form is and must be placed in the ovum germ. It has a distinct destiny, and *carries with it all the attributes of its species.* The lower order of animals have what is called *instinct.* The *great characteristic of man is mind—immortality.* Like the other characteristics of the human germ, it must have its origin there. *The embryo is immortal.* For no new quality can be added to the being at any time during pregnancy. That must be admitted by all. At the very period of conception, and during the passage of the ovum from the ovary to the uterus, it is a distinct, independent, living being, having no direct connection with its *bearer*, and receiving its

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nutriment from a being as distinct as when its little head is pillowed upon its mother's breast, and draws its daily sustenance. If it requires the intervention of law then to protect its body—if it possesses the right to transmit property and holds rights of a similar character—does it not demand the same at any period of gestation?

With this view of the case before us, I suggest it as our imperative duty to direct the attention of legislators to the importance of enacting a statute in conformity thereto. The death of an embryo either by direct physical force or the administration of some noxious substance, should be declared murder in the first degree. For if the *embryo* possesses the *indestructible attributes* of the fully developed being, why should it not be placed under the ægis of the law, to be protected by its conservative power, and thus led to fulfill the purpose designed by its Creator. In the death of a developed human being, the body returns to its mother dust, but the indestructible—the mind—wings its flight to Him who gave it. So with the little *embryo*, when it has laid aside its "mortal coil," it puts on immortality around the throne of its mighty Creator. And there it shall remain to confront the fiend, that may have sent it on its uncalled errand. "Thou shalt not commit murder," is the divine edict that has gone to the uttermost parts of the world, and is engrafted in the laws of all nations.

But it is not necessary for me, at this period of the world's history, to elaborate on the enormity of murder. The consideration of this subject, however, in criminal and civil cases, is often of the first importance. When a woman has the penalty of death resting upon her, and pregnancy is set up as an obstacle to her execution—if she be pregnant at all, not whether she be advanced to the fifth or sixth month, or whether she be "quick with child," as in the English law, but if there be pregnancy, it should be a full and justifiable bar against her execution. For she carries within her a living being, which should not fall under the same sentence.

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In civil cases, many protracted contests concerning the inheritance of property would be obviated. There could be no such diversity of laws in different countries as now exists. The English law requires that the child shall be born alive—*not viable*, in the general acceptation of that term. The law of France requires, not only should it be born alive, but *viable.* Why these discrepancies? What faculties, what attributes does the new born infant possess, which are not common to the *embryo?* If such high privileges are conferred on the infant, that comes into the world but to die, why not to the offspring during gestation? The same blood courses through its tiny veins, and should therefore be entitled to rest its rights upon the same laws of consanguinity.

Endless must be the perplexities which must arise in determining what constitutes *viability.* Though the English law may be preferred from its greater simplicity, controversies may and doubtless do arise on the evidences of life at birth. The exhibition of nervo-dynamic force—the movement of some muscle immediately on its entrance into the world, upon which the evidence of life is based, may take place after death, from some foreign exciting cause, and not be the result of life action.

Then, there may be life without respiration, and thus the little creature, by a superficial observation, may be deprived of its legal rights, because it was not seen to breathe. It will thus be perceived, to what intricate and undeterminable questions the present legal view must lead. But if instead of the requirements of the present laws, evidence of *conception alone* were demanded, it would set aside these difficulties. If a difference of opinion should arise as to what constituted evidence of pregnancy during the early months, it will be definitely settled at a later period. For where the human eye and ear fail to furnish the required information, the microscope can be brought to bear upon the expelled contents of the uterus, upon the teachings of which the utmost reliance may be placed.

There is one more point to which I will direct attention.

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Though last in the arrangement, it can be superseded by none in importance. The view which I have presented of this subject should, in our humble judgment, be inculcated on the popular mind. By a knowledge of this fact, many might be deterred from producing abortion, and many be induced to reject the syren overtures of the seducer. If by such means one may be rescued from the appalling miseries of seduction, from a *deep*, to which there is no bottom, can the human mind compute the value of such a reflection.

I will not farther extend my observations, for I have already transcended my original limits. I trust they may be the means of awakening investigation in others.

Is not the *embryo* a distinct, independent being? Does not the fecundated germ carry with it all the attributes of its species? Does it not possess that immortality which knows no annihilation? If so, to what are we entitled, that it should not be a participant?

ART. III.—*A Case of Small-Pox occurring in King William County.* Reported by DR. WM. P. BRAXTON.

I THINK it due to the public, and to our worthy vaccine agent of Richmond, that I should give publicity to a case of variola, which has recently come under my observation—the manner it was produced, and all the remarkable circumstances attending the case. It is a case that presents new features in the history of vaccine virus, as well as in that of variola.

On the 12th of February I received a scab from Dr. Peticolas, with directions, &c. how the virus should be applied. (And here let me advise him in future to recommend an old lancet to apply the virus with instead of the finger, and thus prevent the waste of a good deal of virus.)

On the 15th of February I vaccinated my two dining room servants. One had been vaccinated before, and had a good cicatrix on his arm. He did not take. The other boy

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took very well, going through the regular form of vaccine fever, which I always like to see—like a nauseous dose of medicine when given to the patient makes them very sick, but the effect is generally good.

On the 16th I vaccinated Mr. Garlick's three youngest daughters, and some of the blacks about the house. All took well—fine scabs produced, which I had his whole lot of negroes vaccinated from.

On the 17th I vaccinated from 15 to 20 persons, and the next morning I was called to Mrs. Pemberton's to visit a case of sickness. While there I was requested to vaccinate her family, which I very promptly acceded to. I saw the subjects on the 3rd and 5th days; doing very well; progressed *pari passu* to the shedding of the scabs, several of which I examined, and advised the other members of the black family to be vaccinated with.

On the 9th of March my neighbor, Wm. M. Turner, while I was visiting a patient at his house, requested me to vaccinate some members of his family, if I had any other matter but that from the Richmond agent. I informed him that I had been able to procure but one scab this season, and that one Dr. Peticolas sent me in February. I had vaccinated some 27 persons, adults and children, with it, and no bad effects so far had been exhibited—the remnant of which scab I then had in my pocket, and if he wished, I would vaccinate his family as far as it would go, assuring him at the same time that I thought it perfectly safe virus. Accordingly I vaccinated one of his sons, a maid servant and a lady then staying in his family in the capacity of a seamstress. She presented very much the appearance of one in ill health—chronic hepatitis personified. I remarked to her, when asked if I thought it would injure her to be vaccinated, that I would rather not vaccinate her, but if she dreaded having the small-pox, I would do so, and jocularly remarked to her that it might have the tendency of bringing about reaction in her system, which looked as though it had been years since a thing of that sort had taken place. Of course I then had no idea of such an *action* as resulted.

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On Thursday night, the 20th, Miss Jane had a severe ague and fever. On Friday morning, the 12th day, the eruption made its appearance. On Saturday she was walking about the house, slightly broken out with what I thought the eruption attendant on some cases of vaccination. I thought very little of it during the day. I visited her again on Tuesday 25th, when, upon examining her more closely, I found she had passed through all the premonitory symptoms of the small-pox, such as languor, drowsiness, pain in the head and loins, attended by pyrexia, feverishness, tenderness of epigastrium, &c.; and it then was clearly a case of confluent small-pox. I commenced treating it as such, and on the 29th I called in Dr. P. Baker to attend the case with me. He was satisfied, as soon as he saw the case, that my diagnosis was correct. The lady has since recovered, and with but few pits or cicatrices to remind her of the awful and alarming malady. Upon investigation I found that the lady had not been any where likely to be subjected to the small-pox infection, nor did any other member of Turner's family take the disease. How can it be accounted for, that out of 32 cases of persons vaccinated, the 32nd one took the small-pox, and the 31st escaped? I very much doubt whether such a problem can be solved. It strikes me as being something new in medicine.

Art. IV.—*Cases from my Note Book and Memory.* By L. Faulkner, M. D. of Halifax County, Virginia.

Midwifery practice furnishes the most interesting cases to a country physician. In these cases life and death are more immediately brought before us, hanging as it were upon our skill and patience, as instruments in the hands of him who has doomed woman to travail in sorrow, with the conditional promise that nevertheless she shall be saved.

Among the difficulties endangering the mother's life,

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retained placenta stands prominent. It is my design to give a case or two of this complication of labor, without farther preface.

1841, July 7.—Saw Mrs. J*** about 1 o'clock P. M. Had been delivered of child four hours; labor pains frequent, forcing out large coagula of blood. A very intelligent midwife thought she had lost two chamber-pots full of blood. She was very much prostrated. The uterus contracted during the pains, but was flaccid in the intervals. Gentle traction by the cord was of no avail. I instituted a brisk friction over the region of the uterus and administered five grains of ergot. Some contraction was soon imparted, and in about an hour the placenta was brought away by repeating the traction at the cord. Kept up the friction for an hour or two to insure against recurrence of hemorrhage.

1843, July 24th, 7½ o'clock, A. M.—Milly, a middle aged servant, had been delivered of a child about 12 hours, by a colored midwife, who had been unable to get away the afterbirth. Labor pains strong, but little hemorrhage; uterus hard and of considerable size, and drawn from one side to the other and forward during the pains; a small portion of the placenta presented through the os uteri; the os internum rigid, and constricting tightly the placenta during the pains. Occasionally two or three fingers could be inserted through the stricture by the presenting portion of the placenta, which was found adherent by a large surface to the walls of the uterus. The slightest traction of the cord induced rigid stricture. Having the fear of Prof. Hodge before my eyes, (he inculcated the almost absolute necessity of taking away the placenta, whilst Dr. W. Harris, our examiner on this branch at the medical institute, dissented from him,) I informed the master of my diagnosis, and requested consultation.

The late Dr. Thomas H. Averett, one of the very best accoucheurs in the country profession, was called in. He was called to her in the preceding confinement, and failed to get the placenta entire. Upon examination, he concurred

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in my diagnosis, that nearly half of it was adherent by morbid attachment. We remained with her from 4 o'clock P. M. until 12 nocte, retaining hold of the placenta alternately an hour or two at a time, endeavoring to overcome the stricture and seize the first opportunity to detach it. During this time 25 grains of ergot were given without benefit. We had to give it up; and as only a small portion of it could be taken away, we determined to leave the whole mass, trusting to the vis medicatrix naturæ. As of course she was much exhausted in this time, quiet and rest were enjoined.

26th, A. M.—Pulse 110; no change in condition; rest; light diet, and tepid fomentations to vulva.

Evening—Pulse 120, and strong; pains frequent; abdomen tender, and rather hot; had a chill late in afternoon.

Venesection one pint, reducing the pulse to 112; a large mush poultice to the abdomen; tepid injections per vag.; 10 drops acet. tinctr. op. ordered.

27th, 7½ o'clock A. M.—Pulse 120, and of sufficient force, sensation of weakness; frequent pains; soon after my arrival a strong one forced out a large portion of the placenta, causing a diminution of the uterine tumor, which was now only felt just above the pubes. The pulse abated to 110.

The weather being very warm, I cautioned the nurse to suffer nothing offensive to remain in the house. Injections per vag.; mucilaginous drinks and diet, and four doses of three grs. each of Dover's powder at intervals of two hours.

Nine o'clock P. M.—Found her cheerful; pulse 94; abdomen natural to the hand; had discharged, she thinks, the last of the after-birth. Evacuated the bowels by enema. Continue the antiphlogistic and antiseptic regimen, vaginal injections, &c.

28th, 6 o'clock P. M.—Has a slight headache; pulse 106, tolerably full and trilling; abdomen relaxed, with a little pain and tenderness just above the pubes; hot skin, and burning of the feet; bowels rather torpid. She has been suckling her infant. Warm elm poultice over hypogas-

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trium. ℞ Calomel, five grs.; Dover's powders, three grs. in addition to previous prescription.

29th, 8 o'clock A. M.—Pain and tenderness much moderated; surface natural; pulse still 104 or 106; medicine has operated kindly; has discharged some shreds of tenacious mucus from the vagina. Applied 25 leeches to the hypogastrium. Continue per vag. injections, and antiphlogistic regimen. Convalescence from this time decided. In this case I suspect the protracted efforts at manual delivery and hot weather combined aggravated the subsequent symptoms.

1846, July 16th.—Whilst at the cabins, I was asked by the servants to examine what proved to be an entirely sound placenta, that had just been discharged by Eveline, a middle aged woman, who had had an abortion of fœtus four or five months advanced, about five weeks previously. She had been up most of this time, and occasionally out in the field, though troubled with repeated hemorrhages from the womb.

1846, August 9.—Saw Paulina, servant, about 21. Found the head of the fœtus in the vagina—delivered it in due time. Pains inadequate to expel the after-birth, and the hand could not be introduced to grasp it. Let her take needed rest during the night, and finding no change next morning, left her, with the same prescription. Of course there was no flooding.

10th, P. M.—Statu quo. Warm fomentations to the abdomen and a dose of ergot were followed by expulsion of the placenta. In such cases I should hereafter try injecting the cord.

Now, from my experience and observation I conclude, that whilst the placenta ought to be taken away promptly when it can be done with safety, yet as there is danger of irritating the womb as much by persevering manipulations of us hard fisted country physicians as by leaving the placenta, it is better to ground arms in proper time, and see that nature has a fair chance, interfering only to aid and

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comfort. Of course, when there is hemorrhage the efforts should be commensurate with the more urgent necessity of speedy delivery. A lamented friend some years since told me once after a hasty consultation with the attending physician in a case of alarming flooding with retained placenta, satisfied of the importance and practicability of taking it away immediately, he was staggered by a grave proposition from the horror stricken husband, upon hearing their decision, that they all unite in prayer for divine assistance before undertaking so momentous an operation. He replied to him that his wife was in imminent danger, but could be easily delivered; though he would not object to his prayer, provided it was short, for there was no time to be lost. They solemnly knelt down, and after a very appropriate prayer in matter and length, if not in time and place, the wife was safely delivered. As firm a believer as I am in our utter dependence upon the Supreme Being for success in every good undertaking, I rather think I should have called up Solomon in this consultation to testify that "there is a time for all things," and suggested that it might be more suitable to strengthen my hands by prayer whilst I was engaged in the work. I should have hardly shown the professional courtesy that he did in taking the attending physician to one side after ascertaining the ease with which the placenta could be removed, to advise the operation. A paramount regard for the patient would have impelled me to complete the delivery while my hand was in. He was, however, scrupulously courteous in his profession, and in this case lost nothing by politeness both to the physician and husband.

ART. V.—*On the Ophthalmoscope and its Uses.* By JOHN H. DIX, M. D. Boston, Massachusetts. No. II. (Continued from p. 377.)

CASES for ophthalmoscopic exploration may be spoken of under two general heads:

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1st. Those which are looked at in the hope of a practical benefit resulting to the eye examined, or to its fellow.

2nd. Cases of hopeless and very considerable blindness, in which no personal inconvenience is likely to arise from the use of the instrument, and where from observations in large numbers and carefully collated, advances may be expected in the treatment of the early and curable forms of some of the many disturbances of vision, now very loosely classed under the comprehensive term "amaurosis."

To these last, my own observations have been chiefly but not exclusively directed, as is probably the case with most persons who have been similarly engaged. From them have been derived nearly all of those cases which I have found most to require engraved illustration, and to them will be devoted nearly all that I have to communicate to you.

But before speaking of these, I wish to say something of the comparatively few cases which may fairly be ranged under the first head, something to establish the claim of practical utility for the ophthalmoscope at this early period of its use.

It is often of immediate use in determining the diagnosis, especially when from the ordinary means of observation, there is a question as to the disturbance of vision being due to the crystalline system or the posterior textures. For the detection of adhesions of the iris or ciliary processes to the capsule, or deposits of lymph or black pigment upon it, constituting what is sometimes called false cataract, I have never found it necessary to resort to artificial light. Dr. Wallace, whose minute researches into the anatomy and functions of the eye, gives great weight to his opinions, does (as will be seen by the note at the conclusion of my first article,) make use of it for this purpose. Dr. E. Jaeger gives an engraving and detailed account of a case of extravasation of blood in the canal of Petet,* a pathological condition which has never been detected by any other means.

* Beitrage sur Pathologie dés Auges von Dr. E. Jaeger. Wien. Tafel 2, fig. 1.

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For the discovery of true cataract of the lens or posterior capsule, the ophthalmoscope is sometimes indispensable. The catoptric method of M. Sanson is ordinarily reliable. It indicates the existence of a very slight general or diffused opacity of the lens, but it does not show or prove the existence of very small patches, specks and lines of opacity either in the lens or capsules.

Such opacities must necessarily, in the catoptric method, escape detection whenever the crossing of two of the three images of the flame happens to coincide with the opacity.

This fact is illustrated by the following case, which I select, because ordinary examination now verifies the examination with the ophthalmoscope:

July 9th, 1855.—Thomas F*** of Medway has, during his whole life, had occasionally pain in his eyes, for the most part provoked by application to his business of harness making. A year ago he began to see before his left eye small black floating muscæ. They have increased in size and number. In May last he had flashes of light before both eyes, followed in a day or two by the appearance of a black spectrum of the size of a large pea before the left eye. This spectrum seems to him only to follow the motions of the globe.

Vision not quite distinct as regards fine print with either eye, and less so with left than right eye. Places his book or work much nearer than formerly, and sees best in a moderate light.

No visible opacity in lens, and the three images of a flame of a candle distinctly reflected.

Frequent headache.

Examination with the Opthalmoscope.

Right eye.—In the posterior part of lens or in the posterior capsule are several minute specks, the largest being towards the nasal side of the pupil.

The specks appear whitish in front of the red glare from the fundus of the globe with the interposition of a lens of two inch focus.

Left eye.—The same appearances, except that the specks are not so far behind the flame of the iris, and are equally distributed.

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In this case the existence of cataract could have been only guessed at without the ophthalmoscope. Dr. S. T. Haven and Dr. J. Sloucing examined and recognized the disease with me.

May 10th, 1856.—Cataract in left eye now clearly visible without dilatation of pupil and without a lens.

It is of still greater practical utility in a case of amaurosis, which presents some equivocal appearance of cataract, to ascertain that no cataract does exist. In the following case of amaurosis, but for the demonstration by the ophthalmoscope, that the lens and capsules were clear, I should not have adopted immediate treatment.

October 17th, 1855.—Mrs. L. J*** of Boston, æt. 56, for 25 years has used convex lenses, the power of which, for close application, has been increased three times. For a year past her vision has been sensibly less than before, and for a few weeks past has rapidly failed. Occasionally, for many years, has had pain or an approach to it in the frontal region, after fatiguing use. For the past year, and especially the summer, there has been a frequent sensation of uneasiness and a disposition to close the eyes, from intolerance of light.

Now she sees best in a moderate light, and often experiences relief from wearing plain colored glasses outside of her lenses.

The most distinct vision of letters without her glasses is 7½ inches. At this distance she sees the word "preface," on the second page of Boston Directory, 1854.

Either there is an amber colored opacity in the crystalline lens, or reflection of similar color from the fundus of each eye, giving the pupils a yellowish, hazy appearance.

With the catoptric examination of M. Sanson, the second and third images are discernable, but so faint as to leave a doubt as to the condition of the crystalline lens.

Examination with the Ophthalmoscope.

Right eye.—The blood vessels and entrance to optic nerve are as distinctly visible as I have ever seen them. The general surface of the retina less florid than in health, but no other abnormal condition visible. Of course this proves conclusively that there is no opacity of the crystalline lens, and that the disease is amaurosis.

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The left eye presenting without precisely the same appearance as the right, I do not examine it with the ophthalmoscope.

Mrs. J*** has been and is still under treatment. Feb. 20 was this memorandum. To-day without glasses reads fine print on same page as easily as word "preface" formerly. The type of that word is at least four times the size of the fine print.

After an operation for artificial pupil, some six months ago, I found that although a large and apparently clear black pupil was formed, the patient had only a perception of light as before the operation. It was possible that a portion of black pigment remained behind, the opening in the iris giving an appearance of blackness, but obstructing vision. With the ophthalmoscope I could see the retina, and satisfied myself, if not the patient, that the present blindness is owing to other causes than the want of a pupil.

LECTURES AND TRANSLATIONS.

I. "*Clinique de la Faculté.*" Paris. M. NELATON, Professor and Surgeon. Notes by ST. GEORGE PEACHY, M. D. Richmond, Va.

Recto-Vaginal and Urinary Fistulæ.

1. If a section corresponding with the plane of the median line be made through the pelvic viscera, it will be found that the posterior wall of the vagina and anterior wall of the rectum are not equidistant one from the other, throughout the entire length of the former.

2. The superior fourth of the vagina is separated from the rectum by the peritoneal reflection or *recto-vaginal cul-de-sac*, as it is termed.

3. The two middle fourths of the vagina are in simple contact with the rectum, loose areolar tissue and vessels only serving as bonds of union.

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4. The inferior fourth here begins; the rectum curves downwards and backwards; the vagina upwards and forwards, the interspace between the two being quite accurately represented by an inverted y, thus ʎ, the two arms of the letter limiting it, while the stem represents the mere contact between the anterior wall of the one, (rectum,) and the posterior wall of the other, (vagina.)

5. These two cavities, then, in their relations with each, are divided into three portions, upper, middle and lower; and a just appreciation of them must be entertained in order to comprehend fully the character of fistulæ occurring in each one—recto-vaginal fistulæ.

These fistulæ may be occasioned by actual rupture, by wound from bones of a child's head or other portions of body, by long sojourn of head in pelvis, creating pressure followed by cessation of circulation, sloughing, fall of eschar, and finally terminating in an aperture.

A fistula thus produced may take place high up, in which case it must traverse the two folds of the peritoneum—in other words, penetrate the recto-vaginal cul-de-sac. Another may occur in the middle portion, having simply to thread its way through only the thickness of the opposing walls. A third, finally, may appear in the lower portion, and present a longer route through the perineal tissues filling up the interspace before alluded to and described. Hence these fistulæ assume different lengths in consequence of the disposition of the parts involved, which disposition has given rise to the three distinct orders of recto-vaginal fistulæ, and upon which their curability mainly depends.

In regard to their curability, they may be compared to those occurring in man, which M. Nélaton terms urethro-perineal and urethro-penial, according as they occur posteriorly or anteriorly to the scrotum. Their order, however, is inverse; for the former, in women, are more curable the lower down they present themselves—i. e. nearer the external orifices of the vagina and rectum, whereas the latter, in men, are less curable the nearer they approach the external orifice of the urethral canal. The reason for this is, that a fistula occurring in parts thin and hardened by previous inflammation, approaches nearer the character of a hole made in a membrane, edges indurated, nonsuppurating, nongranulated; the route (trajet) is of little depth; the walls do not tend to approximation, but are kept apart by the tension of the fibrous ring encircling the orifice. Such is the character of the fistula occurring high in the vagina, perforating the peritoneal reflexion, and finally entering the rectum. This,

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then, may be termed a membranous fistula characterized as above, by indurated edges, by want of granular suppurating surface, and in which the walls do not incline to approach each other.

Descending lower down, however, we find that the nearer the fistula be to the external orifices of the vagina and rectum, the more it must traverse the perineal tissues; must consequently be longer and probably tortuous—all of which circumstances concur in rendering it very different from the former kind. This latter variety cures spontaneously, says M. Nélaton. 1. Because they are longer. 2. Because their walls are carpeted with pyogenic granulations. 3. Because they tend to approach and seek adhesions. 4. Because they are often tortuous, and this favors approximation of surfaces. 5. Because they are generally oblique—and if (as in case now in the wards) the obliquity works from before, (vagina) downwards and backwards, (rectum) a sort of valve is formed, in the bladder where the ureters enter, and this preventing introduction of fœcal matter into the fistulous track, expedites wonderfully the cure.

The peritoneum, in consequence of its properties as serous membrane, very often prevents the passage of fistulæ through its two layers; becoming inflamed, plastic lymph is thrown out, and thus affords a barrier to fistulous progress; hence membranous fistulæ are fortunately of rare occurrence. Sometimes, however, the peritoneum, after adhesion of its two surfaces, becomes subject to a still further grade of inflammation, and is perforated. The treatment to be pursued in such cases will be discussed hereafter, and compared with the various modes of cure heretofore employed.

M. Nélaton insists with great emphasis upon the spontaneous cure of recto-vaginal fistula occurring below the recto-vaginal cul-de-sac. I examined the case giving rise to the above clinical lecture, with Dr. Paul Lorain Interne at "La Clinique de la Faculté," who, at that time, was giving private bedside instruction in M. Nélaton's wards. The fistula was low down; two fingers, one in the rectum, the other in vagina, were only separated by the valvular septum hanging over the oblique canal. A large sound could easily be passed from one cavity into the other. The patient insisted upon an operation, but M. Nélaton dismissed her for ten days, saying that he would operate at the expiration of that time. He expected, however, to find a spontaneous cure upon her return. Patient entered the wards December 18, 1854, and upon examination December 27, 1854, by M. Nélaton, the fistula was found to have entirely closed; fœces were passed

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per rectum alone, and were moreover liquid. The length of time this fistula had existed before patient's entrance was not ascertained.

Vesico-vaginal and Vesico-uterine Fistulæ.

No. 19. Female ward. "Clinique de la Faculte." January 10, 1855. Paris.—This case is the result of a protracted labor. The woman has had three accouchements. The first was accomplished by means of forceps. The second lasted three days. The third was a shoulder presentation; two fruitless efforts by means of instruments were made to deliver the child whole; finally cephalotripsy was had recourse to. The pelvis is constricted, child's head creating pressure against anterior vaginal walls; suspended circulation in parts; gangrene supervened; eschar fell five days after delivery, and the woman for the first time observed that she was passing her urine by unnatural ways. The fistula is seated between the bladder on one side and the neck of the uterus on the other. The finger being carried into the os uteri, as can easily be done, in consequence of fissure in its posterior lip, created during labor, feels a sort of infundibuliform depression, in which exists the uterine orifice of the fistula. An articulated sound carried into the bladder and entering the vesical orifice may be brought in contact with the finger. If the speculum be introduced as far as the os uteri, and kept there a sufficient time, drops of limpid urine may be seen to escape therefrom. If an injection of milk and water be thrown into the bladder while the speculum remains in position, or be thereafter introduced, the turbid fluid will make its appearance through the fistula after a time. Many a case has occurred in which these injections have been thrown into the bladder, and nothing of the kind seen to pass through the other orifice of the fistula. Either a cure or nonexistence of fistula has been concluded from this fact, whereas, if proper attention had been paid in introducing the injection, and an inspection per speculum been kept up sufficiently long, the fistulous track would have been discovered.

Is this a case for operation at present? M. Nélaton thinks not, and cites, in favor of abstaining from immediate interference, four cases, in which a cure has taken place spontaneously, four months being the shortest period elapsing between the cure and discovery of fistula or its production at the time of accouchement. But in spite of this, the general rule is, "persistence of the fistula." In one of these cases M. Nélaton was occupied while acting surgeon at

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Hôpital St. Louis. The fistula measured from two-fifths to four-fifths of an inch across its vaginal aperture. He postponed operating from day to day, and in the mean time consulted distinguished surgeons of Paris. The other cases belonged to M. Marjolin and M. Dagneau. After postponing in this way, the fistula cured spontaneously, and M. Nélaton only asks that the merit of not preventing a cure by operation be accorded him. The other three cases mentioned were also of spontaneous cure.

The progress made by the case now in the wards is highly satisfactory. At first, the patient passed her urine entirely through the fistula, and experienced no desire to relieve the tension of her bladder. She now has a besoin d'uriner, and though much still passes per vaginam, she nevertheless expels some little per urethram at end of urination. Another circumstance favoring abstinence of knife is, that before the patient came from M. Paul Dubois' wards, this latter proved existence of two fistula—one urethro or vesico-vaginal, the other vesico-uterine. The vesico-vaginal fistula no longer exists, and the vesico-uterine has assumed a valvular form, much insisted upon by M. Nélaton as a favorable sign of progress. The circumstances of form, of favorable progress, of former existence of two fistulæ, of obliquity which the remaining assumes, lead M. Nélaton to abstain from operating.

Urinary fistula may be divided into, 1. Fistulæ vesico-vaginales. 2. Fistulæ vesico-utero-vaginales. 3. Fistulæ vesico-uterinæ.

Fistulæ vesico-vaginales.—It has been proved above, that sometimes urinary fistulæ cure spontaneously; that the general rule should be "persistence of fistula." The few cases quoted are striking illustrations of the spontaneous cure. The exact limit of time beyond which spontaneous cure becomes impossible, and the exact limit of extent occupied by the fistula beyond which it never cures spontaneously, are alike unknown at present. There may probably be such limits. Why not? M. Nélaton does not venture to give them.

The treatment of these fistulæ has for a long time occupied surgeons. Each one has employed his favorite method of cure, but none have been attended with notable success quoad the number of cases operated. Cauterization with the red-hot iron was Dupuytren's favorite remedy. M. Nélaton has assisted at his operations, watched the result, and seen nothing to dispose him to declare in its favor. Vivifying and approximating the edges are another means quite

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proper in small fistulæ occurring in supple tissue, but fails often. This cannot be sustained dogmatically. Anaplasty has likewise had its advocates, and at first the most magnificent results were published; but it is enough to say that it has passed into oblivion, to prove the utter inutility and falsity of the reported cases. Among the numerous operations of this sort, there is but a sole case of success. Jobert (de Lamballe) has published his cases and his operations too, and they, like all the others, "have been attended with great success," while very few if any of the fistulæ have been cured. Jobert's operation consists in making a curvilinear incision of the utero-vaginal cul-de-sac where the anterior lip of the os uteri joins the vagina. This releases, as it were, the portion of vagina superior to the fistulæ, and allows approximation of the edges without much tension, thus fulfilling the most important desideratum in such operations. This is very well to talk about, and gives occasion for a pretty clinique; but as to the operation itself, when it becomes to be performed, the fact becomes different, and Jobert finds himself operating in a long, deep cavity, whose walls are incessantly tending to collapse, which require tension, badly applied at best, to keep them apart, and where the operator himself can only grope his way. M. Nélaton, in speaking of these operations done in the dark, says, "one cannot do what one wishes, but only what one can, under such circumstances." The operation may be beautiful in description, but impossible in performance. Such is Jobert's operation, which, if performed easily and without hindrance, would not inspire M. N. with confidence in consequence of the nature of the tissue in which it is performed.

Having said thus much of these operations and their want of success, we next pass on to the nature of the tissue in which they are performed, the circumstances under which they are performed, the fact of the urine passing over the site of operation, the necessary ulceration of the part ligated, and finally the difficulty of proving the cure, owing to the desire of the patient to believe herself cured, and the surgeon relying upon her say so, or to the length of time and pertinacity required to detect the still existence of fistulæ after operation.

Firstly—The tissue operated upon in vesico-vaginal fistulæ is entirely different from that in which fistulæ occur in other parts of the system. Add to this the circumstances of pressure, of inflammation, ulceration, gangrene, fall of eschar, and the consequent hardness of the edges and surrounding

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parts, the thinness of the walls, making it almost a membranous hole; and we have ample reason to believe that this tissue, presenting normally an indisposition to agglutination of ruptured surfaces, has this indisposition greatly enhanced in consequence of the alteration of structure it must undergo in the various steps of morbid progress giving rise to the fistulous orifice.

Secondly—The urine must pass over the site of operation. No catheter can prevent it, in as much as after a residence of the catheter, for a certain limited time, the walls embracing it give way, and the urine passes between them and the instrument. Now, this passage of urine over recently cut surface is detrimental; and supposing that the parts become agglutinated before infiltration occurs, we must remember, that

Thirdly—The ligatures must cause ulceration, and that the urine, passing in contact with the ulcerated surface, penetrates therein, and destroys the slight adhesion that may have theretofore taken place.

Fourthly—How many cases of cure have been published that were not cures? Frequently the surgeons who report them are to blame, because they have either failed to make a proper examination of the result, or have relied upon the say so of the patient, who, wishing to hold her footing of physical equality with all others of her sex, declares herself nominally or virtually cured. Therefore, it will not do to rely upon the patient's account of herself. We must not consider all the published cases as presenting all the desirable guarantees of cure; and in the examination made after operation, we should proceed as if it were with a desire to prove a nonsuccess.

In these operations, as we have said, we are compelled to grope our way, for the disposition of the parts is such that one cannot operate under full light. Moreover, the success of the operation is most frequently null in consequence of the nature of the tissue operated upon, inodular, of slight depth, having little tendency to agglutination of surfaces, and in consequence also of our working in obscurity in making the section between the bas fond of the bladder and the vagina, a thing almost invariably followed by infiltration of urine, tending to separate all adhesions.

Whenever fistulæ are operated upon, we should never forget *how* a good result can be obtained, manifesting itself in perfect cure. It is not by simple adhesion—it is rather by granulations which form from day to day, and fill up the fistulous track. Now, in the fistulæ we have discussed that

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anatomical disposition which gives rise to pyogenic granulations fails, and these latter not existing, there cannot consequently exist the reparative tissue which should fill up the fistulæ.

Fistulæ vesico-utero vaginales.—Jobert's operation in these cases is to incise the inferior margin of the fistula and posterior lip of the os uteri, to bring the two together, and produce agglutination. In this way he attempts to obliterate the connection between vagina and uterus, to convert them into separate cavities, and to cause the menstrual flux to pass by the bladder and urethra, thus making the uterus and bladder "a grand compound receptacle of the urine and menstrual secretion." M. Nélaton rejects the operation in itself as attended with evil consequences if successful, and moreover does not believe in its success in consequence of the nature of the tissue before discussed, and the disposition of the posterior lip to spring back to its natural position, and thus destroy adhesions. "How (says M. N.) can one approximate this posterior lip of the uterine mouth to the inferior border of the fistula when it is not more than two-fifths of an inch in length, and often not so much, and when its nature is such that it always tends to regain its normal position? How can such a thing be done in this immense utero-vaginal cavity?" There is not a case of cure formally demonstrated of this variety, but many deaths have occurred in consequence of hemorrhage.

Fistulæ vesico-uterinæ.—Jobert imitating Vidal, incises the lips of the os uteri, brings them together by suture, and attempts to obliterate the uterine cavity, or at least to sever all connection between the uterus and vagina, giving the menstrual flux a passage by the bladder and urethra. The remarks above made apply here, and no cures have been obtained.

M. N. finally institutes a comparison between the case as it now (January 21st) presents itself in the wards, and the effects which might have resulted from an operation. The woman is doing well; fistula difficult of recognition; passes no urine through vagina, day or night, involuntarily; has a desire to urinate, and then passes a considerably larger portion per urethram; sound with articulated extremity is necessary to enter obliquity before mentioned. All this would have been attributed to operation. He awaits the case. It would be a great surgical misdemeanor to interfere at this stage. If interference becomes necessary, he will use the electric cautery and a small pessary between anterior and posterior lips of the os uteri, and let remain a catheter not to prevent infiltration of urine but *distension of bladder.*

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Resumé of the Condition of Patients.

No. 19. Female wards. January 23, 1855.—Forty-two days have elapsed since giving birth to a child. During 15 days totality of urine was passed by false routes; later, a desire to urinate was experienced; *at present*, but little passed per vaginum. This desire is felt every two hours of the day; patient relieves it without losing any considerable portion per vaginum. The same is experienced every three hours of night, and the same happy result is manifested. How can we esteem operations after this spontaneous and satisfactory progress?

No. 19. Female wards. April 18th, 1855.—Patient thoroughly examined by M. N. The woman is cured spontaneously. This is result of noninterference on part of surgeon. Yet it must be remarked that not all cases terminate so favorably. It will be seen from the foregoing what cases may be deemed curable by spontaneous action in the parts involved, as well as those which from their date and apparent defiance of time called for surgical aid. In these latter, M. Nélaton employs the electric cautery, &c. as before described.

June 6th, 1855.—In speaking of vesico-vaginal and vesico-uterine fistulæ, three cases of which are now in wards, (one vesico-vaginal, the other vesico-uterine,) M. N. remarks that a case of apparent vesico-uterine fistula may occur without being such actually. Bérard once had a case in which, unfortunately for the patient, autopsy revealed a perforation existing between the ureter and uterus. Inflammation having been set up, a fistulous communication was established between the two, and the ureter being occluded towards its vesical aspect, the renal end poured its contents into the uterus. Of course, no injection in this case thrown into the bladder could have made its escape per uteram et vaginam. Autopsy alone proved the nature of the fistula.

Allusion was again made to the numerous and unsuccessful operations which have been suggested for the cure of these fistulæ, vesico-vaginal and vesico-uterine—and from the nature of the case, M. N. determined to apply the electric cautery to the vesico-vaginal. The other two were making very satisfactory progress.

July 1855.—The action of the cautery was entirely satisfactory—fistula approaches fast a perfect closure. The others are doing well.

M. Nélaton passed sentence of disapprobation upon the greater part of the operations, such as Vidal's, which consists

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in closing vagina in vesico-vaginal fistulæ, by making large and free incisions of vulvar circumference, and uniting cut surfaces by means of suture. This failed; no perfect closure resulted. Then the white-hot iron was used by M. V. with an equally unsuccessful result. Then came Bérard's method of incising the inferior portion of vagina (sphincter) circularly, separating it from surrounding parts, and thus making it an isolated vaginal tube—inner surface mucus, outer surface a bleeding dissection. This done, Bérard would invaginate the vagina, and thus bring in close and lengthy contact the freshly cut surfaces previously separated from outer connections, and terminate his operation by sutures adjusted to this vaginal stump. This failed! The vagina would not remain closed, and the operation was followed by death. Jobert seizing upon the idea of Vidal, bethought himself of his operation, in vesico-uterine fistulæ, whose object is to close the uterine cavity alone, and leave the unhappy woman in the enjoyment of a vagina at least. All are alike unsuccessful. Those most favored by nature sometimes get well in spite of the operation. The operations of Vidal and Bérard are modifications of others hunted out of the dusty archives of unsuccessful surgery, but reported to have been entirely successful. If successful in fact, why slumber so long in oblivion, while each day demands more loudly than the preceding such success? If agglutination or ample granulation of the opposed surfaces were obtained, what must be the result of sojourn of urine in vagina and uterus, if menstrual flux in bladder? The whole vaginal surface must become graveled. What is the result of this? It is needless to answer.

Aug.—Cases make favorable progress. Condition is satisfactory. Here the session closed, and clinical lectures terminated at hospitals of Paris.

II. *Lectures on Varicella, Variola, and Variolous Diseases.* By Armand Trousseau, Professor of Clinical Medicine at Hôtel-Dieu of Paris.

Lecture IV.—Revaccination, Inoculation.

Every one knows how vaccination is performed. Virus is taken from a child's arm, and inserted by from three to six punctures in each arm of the person to be vaccinated. We shall see hereafter that the number of punctures is not a

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matter of indifference. The inoculation once made, if the conditions of the subject and of the virus are normal, nothing is observed during the first three days, except a little redness about the punctures, which disappears a few hours after the insertion of the virus. On the fourth day, a little red pimple is seen, which becomes more elevated on the fifth day. On the sixth day, there is a vesicle at the summit of the papula, which still enlarges, and, on the seventh day, becomes umbilicated. On the eighth day, an inflammatory areola begins to form, and surrounds the vesicle with a red zone, which increases in width on the ninth day, after which it begins to diminish. In the mean time the vesicle becomes opaque, and its fluid contents become turbid. On the eleventh day a crust begins to form; the pustule gradually dries up, and, about the fifteenth day, the scab falls off,* leaving a cicatrix resembling that of a pustule of ecthyma. Such is vaccinia as we see it now, developing the local symptoms I have delineated, and the general phenomena I am about to describe.

But kine-pox, inoculated while it retains all its virulence, acts much more energetically. The pustule is larger, the liquid more opalescent, the areola more extended, and the crust does not fall until the twentieth or twenty-second day. There is a great difference between kine-pox of the second or third generation, so to speak, and that of the hundredth.

On the fifth day, if the child who has been vaccinated is closely observed, some slight disorder of health will be noticed—a disorder characterized sometimes by diarrhœa and languor, sometimes by a little fretfulness merely. This disorder lasts half a day or a day at the longest. On the sixth and seventh days, nothing notable is observed, but towards the end of the eighth day, when the glands of the axilla begin to enlarge and the areola forms, a febrile movement arises, which vaccine physicians call the *vaccinal fever*, to which they attach great importance. This fever lasts two or three days; it is more intense after vaccinations with the regenerated vaccine virus, and is proportioned to the age of the virus and the number of the pustules; it is inversely as the age of the matter, and directly proportionate to the number of the pustules.

A complication sometimes supervenes in the course of vaccinia, which occasionally acquires considerable impor-

* This is certainly a very early date for the separation of the crust; at least five days earlier, we should think, than the average in this country.—*Translator.*

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tance. I refer to a general eruption, termed the *vaccinal exanthem.* It is rare in winter, except in children who are too warmly clothed, but common enough in the summer season. It supervenes from the ninth to the eleventh day of vaccinia; is morbilliform, that is, it presents the appearance of measles; less frequently it is scarlatiniform; sometimes, though very rarely, it is pustular.* I have observed two cases of the pustular variety of the vaccinal exanthem. In one of these cases, there were pimples on the child's skin before he was vaccinated, which he may have inoculated in scratching them. The morbillous and scarlatinous forms are not uncommon in children who sweat freely during the vaccinal fever.

This is the way true vaccinia manifests itself—for there is a true and a false vaccinia—and the latter has a different course. It is to normal vaccinia what varioloid is to variola. When a child already properly vaccinated is inoculated with vaccine virus, and the matter takes, a bright redness sometimes supervenes about the puncture, and in a day or two a pointed pimple forms, which itches terribly. This is a vaccinal eruption modified by the antecedent vaccinia, a vaccinoid. The pimple may be converted into a pustule; but instead of increasing until the eleventh day, the vaccinoid pustule attains its apogee on the fifth day, and desiccates on the seventh.

The cicatrices which succeed vaccination have excited much discussion. It was a rule in England that, only when virus should be admitted to be good a small, round, regular scar, a patch like a normal peyer's gland. When a mark was irregular, elongated rather than circular, or faintly marked, like the scar resulting from a bulla of pemphigus, it was said that the virus that produced this mark was poor and not protective. During the modern small-pox epidemics in France and Germany, physicians paid much attention to this question of cicatrices. In the revaccination of the troops in Prussia and Wurtemberg, only those recruits who had defective or irregular cicatrices were revaccinated at first. But it was found that small-pox did not respect the soldiers with regular cicatrices any more than the others, and that revaccinations succeeded as well in one case as in the other. Since then the matter of cicatrices has lost its importance.

* These observations are of interest in connection with the descriptions of "Remarkable Results of Vaccination," by Drs. Patteson and Parker, in the April and May numbers of the Journal.—*Translator.*

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Protective power.—This question comprises the past, the present and the future of vaccination. Has it protected? Does it protect? Will it protect? Has vaccinia proved a certain preventive of small-pox? From 1800 to 1816, to have proposed such a question would have been considered almost blasphemous. By common assent, vaccinia was as sure a preventive as variola. About 1816, it began to be admitted that cases of small-pox after vaccination were not rare or exceptional, but that a considerable number of such examples might be adduced. Then came the epidemics of 1819 and 1824, which made numerous victims among the vaccinated. At Marseilles alone, of 30,000 vaccinated persons, 2,000 had small-pox. It was necessary, therefore, to admit that the protective influence of vaccinia was relative rather than absolute; and the Jennerian society itself acknowledged this fact. Was this relative immunity real?—for confidence in vaccination once shaken, physicians became sceptical; the epidemics of France, England and Germany proved incontestably that it was real. It was observed that when the variolous poison attacked whole populations, vaccinated persons took varioloid, and the unvaccinated true small-pox. The latter were affected almost universally, whereas multitudes of vaccinated persons escaped altogether, and the remainder had a mild and modified form of the disease. The mortality of the vaccinated was one in fifty, of the unvaccinated, one in four. It was evident, then, that vaccinia afforded a relative immunity. It had been claimed that the protective influence of vaccinia was equal to that of antecedent variola; but, in the epidemic of Marseilles, only $\frac{1}{69}$th of the patients had suffered a former attack of small-pox, whereas $\frac{1}{7}$th had previously been vaccinated. Whence it appeared that the preservative influence of vaccination was greatly less than that of variola.

What is the cause of this imperfect protective power? If we consider in the first place, the inoculation of false vaccinia, we find that it affords the individual no more immunity than an inoculation with the contents of a pustule of ecthyma. The production of false vaccinia is due either to an alteration of the virus or to an insusceptibility of the subject.

Jenner and Gregory noticed the anomalies of vaccinia in certain individuals, and attributed them to insusceptibility on the part of the subject. Here, then, is one cause of non-preservation, a cause inherent to the individual, and due to some unknown abnormal disposition of the economy.

Now let us examine the preservative power of legitimate

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vaccinia. This depends chiefly on two conditions: the age of the virus used, and the time which has elapsed since vaccination.

It was observed in England that the individuals who were vaccinated soon after Jenner's great discovery, in 1798 to 1804, were generally proof against attacks of variola. It was observed also in France, during the later epidemics, that persons vaccinated in 1801–2–3–4, had greater security than those vaccinated in 1816–1820, etc. It was also satisfactorily determined that persons vaccinated with the new cow-pox were better protected than those vaccinated with the virus which had been transmitted through hundreds of children. The age of vaccinia, then, is one cause of its losing its protective power.

The time that has elapsed since vaccination, is another important element to be considered in estimating its protective power. During the epidemics of variola of this century, careful and intelligent physicians have observed that children vaccinated two, three, six, eight and ten years previously, were affected with varioloid, but they noted also that the proportion of children affected increased with the remoteness of the date of vaccination. They justly concluded that the longer a person had been vaccinated, the less complete was his immunity; so that he was more likely to have small-pox, other things being equal, 15 years after vaccination than 10 years after; more liable to be infected at 10 years than at five years, and so on, the number of the vaccinal pustules and the other circumstances being the same.

These considerations lead us naturally to the question of the *degeneration* of vaccinia, a most serious and important subject in medicine.

Jenner was never blinded by his brilliant discovery. He comprehended all its importance, undoubtedly; but, long before his death, he foresaw the probable diminution in the protective power of vaccination. If we compare the old kine-pox with the new, the vaccinia of 1824 with that of 1801, or with that renewed by Dr. Steinbrenner of Alsace, in 1845, we find their effects very different. Let any one compare, by means of the drawings that have been preserved, the vaccinia of 1799 and that of 1824; it is no longer the same thing. The pustules of the latter are smaller, the areola is less extensive, and desiccation is earlier; while the crusts are ready to separate from the pustules resulting from the inoculation of the virus of 1824 at the fifteenth day, in the vaccinia of 1799, the scabs are still adherent at the end of three weeks. The pustules of the regene-

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rated vaccinia, on the other hand, are in all respects conformable to those of the primitive cow-pox. The age of vaccinia is, then, a powerful cause of degeneration. But there are other causes of deterioration, due to the transmission of virus. Read the history of syphilis; see what it once was and what it is now. The syphilis of the fifteenth and sixteenth centuries was characterized by terrible symptoms, by no means comparable to what we now observe. Yet mercurial preparations were employed in the sixteenth century at the onset of secondary symptoms as they are now. Notwithstanding this, the ravages of syphilis were frightful, to an extent which is unknown, or at least extremely uncommon in our day. What is the cause of this change? It depends on the fact that syphilis has been repeatedly transmitted under peculiar conditions that have modified its virulence. When a man or woman has the pox, he or she does not run the risk immediately of contaminating a person of the opposite sex; precautions are used—so the period of greatest virulence is usually suffered to go by, and when, after a time, coitus is indulged in, the virus is transmitted in a state of diminished energy. Syphilis is transmitted by chance or accident, not according to rule, or with a view to perpetuate the genuine and legitimate disease. It results, that after innumerable transmissions, syphilis becomes much milder than it was in the fifteenth century.

When you read the history of epidemics of whooping-cough in the middle ages, you are struck by the formidable gravity of the disease at that epoch. Now a days, whooping-cough is often a serious affair, no doubt, but it is far from being such a severe affliction as it was at the time of which we speak. And the reason is that the unknown virus which produces whooping-cough has been attenuated by repeated transmissions.

Certain contagious diseases have disappeared. The leprosy, that disease which was the terror of the people of the middle ages, is no longer dreaded by any one.

The virus that is transmitted from man to man, undergoes, then, an attenuation or diminution of energy.

Certain causes may render a virus more or less active. Two facts will elucidate this fact. Jenner said: Vaccine virus is most efficient from the sixth to the seventh day. If you take vaccine lymph on the sixth day, there will be but little, but that little will be active. It will be less energetic on the seventh day, though still very active. Afterwards, it gradually loses its virulent property. Jenner recommended that virus should be taken from the vesicle on the sixth day,

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or on the seventh day at the latest. Now, at most of our public institutions, vaccinations are practiced once a week. The children that are brought are vaccinated with matter from the arms of the children vaccinated the week before, that is with virus at the eighth day, the efficiency of which has diminished.

The energy of virus may be diminished by imitating the conduct of agriculturists in the acclimation and domestication of plants. A proprietor in the Crimea, whose flocks were decimated by the tag sore, which is the small-pox of sheep, determined to endeavor to produce an eruption as benignant as vaccinia by inoculating this pustular disease. He divided his sheep into lots of forty or fifty, and placed them in separate parks. He inoculated forty sheep and gave them the tag sore. He selected one which had the fewest pustules, and took pus from this animal, with which he inoculated 40 other sheep. He found that the second lot suffered less than the first. Selecting again the least sick of the flock, he took matter with which to inoculate a third flock. By the sixth transmission, there was no case of death from the disease. Thus, by successive inoculations, the virulence of the tag sore was diminished. After a time, our experimenter had only a single pustule, the pustule of inoculation, with no general eruption. Thus, by going about it skillfully, virus may be educated, so to speak, and its energy diminished at will. The inoculators of the last century always took virus from individuals who had a mild form of variola. At last their inoculations were almost as benign as vaccinations. There is no doubt, then, but that virus may be made to deteriorate and lose its energy by careful management.

Cow-pox is not a disease of man; it is not a human virus; it is an animal virus transplanted into a locality which is not its own. It acts as plants do when removed from their natural soil; it degenerates. See, in our country the chiendent, (couch-grass, *triticum repers*,) it always thrives, for it is on its native soil. But, on the other hand, look at the barley, an exotic plant; unless assiduous and careful culture prevents, it rapidly degenerates. So kine-pox, an exotic, bovine seed, implanted in man, that is to say in a foreign soil, must be sedulously cared for, to prevent its gradual deterioration. It must have faithful culture in order to afford the results expected of it. It is necessary that the virus should be taken on the sixth day of the eruption, or the seventh at the latest, and that a considerable number of punctures should be made, four or five on each arm, in order

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to develop a well marked vaccinal fever. The French and English physicians who remained faithful to the old method of general punctures, were likewise those who the longest combatted the idea that vaccination was not completely preservative.

CHRONICLE OF MEDICAL SCIENCE.

PHYSIOLOGY.

1. *The Physiology of the Spinal Marrow.* DOWLER VS. BROWN-SEQUARD.

The brilliant experiments of M. Brown-Séquard on the functions of the spinal cord, which have lately received the approbation of the French school of physiology, and to which the academiè des sciences awarded the prize for experimental physiology, have been alluded to in our journal. The beautiful theories of Sir Charles Bell, so long accepted by physiologists as to the functions of the anterior and posterior columns of the spinal marrow, have been suddenly and completely upset by M. Brown-Séquard, and what was once order and symmetry has been again brought back to chaos.

These events have elicited a circular from Dr. Bennet Dowler of New Orleans, in which he claims priority of discovery over the French physiologist on this important subject; and whilst we are not now prepared to decide upon the right of either of the contending parties to this discovery, we are certainly desirous that our countryman should receive all the credit which he deserves, and we therefore publish the conclusions of Brown-Séquard, as laid down in the report of the committee of the French academy, and also give the opinions of Dr. Dowler upon this point, as published about twelve years ago. The reader will perceive that in many particulars the two theories assimilate.

Dr. Brown-Séquard concludes his experiments with the following summary:

1. The posterior columns may be divided without destruction either of sensation or motion.

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2. Sensation and motion are destroyed when the gray substance is cut across.

3. Integrity of the antero-lateral columns does not interfere with the loss of motion, nor does integrity of the posterior columns prevent loss of sensation.

4. Division of the posterior fibres of the cord, so far from abolishing sensation in the parts to which these fibres are distributed, appears, on the contrary, greatly to increase it.

5. When the posterior columns are divided, sensation continues to be transmitted between the lower portion and the gray substance, which transmits the impression to the sensorium by means of fibres descending from the upper portion, and joining obliquely the gray substance below the point where the section is made.

Our limits forbid us to detail the experiments upon which the above conclusions are founded. They have been repeated over and over again with the same results, in the presence of a committee appointed by the Societe de Biologie, consisting of MM. Claude Bernard, Bouley, Broca, Giraldès, Goubaux and Vulpian, to whom was referred M. Brown-Séquard's memoir, and who were entirely satisfied with his conclusions. The interesting report which they made to the society is the most convincing evidence of M. Brown-Séquard's skill as an experimenter and his eminence as a physiologist.

The reader will perceive from this extract, which we owe to the Boston Journal, that Sir Charles Bell's theory of the functions of the spinal columns is utterly overturned, whilst the careful scrutiny of two committees of the French societies has failed to discover any defect in the experimentation of the author.

Now, Dr. Dowler claims that he has been inexcusably and unjustly, &c. &c. defrauded of his right as the original discoverer of the very functions of the spinal cord to which M. Brown-Séquard now lays claim. Dr. Dowler, at the time we mention, subjected many animals to decapitation, divided their spinal cords transversely and longitudinally without destroying sensation (?) or voluntary motion. In his own words, "The posterior and anterior roots of the spinal cord were irritated, divided and destroyed; the nerves were followed to their distributions, and dissected away, showing that all of the fundamental principles of Bell and Magendie, concerning the two sets, as well as Hall's four sets of nerves, were erroneous—that the muscles possess an inherent power of contraction; that both the posterior and anterior nerve-roots give indifferently the signs of motion and sensation; that the twitching motions described by Bell and Magendie as exclusively pertaining to the anterior root, can always, contrary to their theory, be more fully produced after separation from the cord than before, by following the nerve towards its distribution; that the spinal cord can be divided in three or four places, and that the head may be removed without destroying sensation and voluntary motion, and that sometimes, in the alligator at least, the head on the one hand, and the separated body on the other, act in a voluntary manner, each for itself giving all the rational indications that could be expected of an animal, as purpose, feeling, &c."—*Charleston Med. Journal.*

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2. *On the Physiological Action of the Anæsthetic Agents.* By Dr. DETMOLD of New York.

An interesting discussion was originated in the New York academy of medicine at a recent session, based upon a paper on the anæsthetic influence of carbonic acid gas, submitted to the academy by Dr. Simpson of Edinburgh.

In the course of the discussion, Dr. Detmold favored the academy with a written exposition of his views of the rationale of the action of chloroform, sulph. ether, and nitrous oxide, the three agents employed for the purpose of producing anæsthesia. He attributes the action of all of them to the production of carbonic acid gas in the system. The first two supply the carbon, which absorbing oxygen from the blood, and the last supplying oxygen, which absorbing carbon, in either case carbonic acid is the result, which by its action on the living organism produces anæsthesia. This theory, though not absolutely susceptible of demonstration, is yet apparently based on a logical foundation, and finds a seeming confirmation in a number of well known facts.

Dr. Griscom stated, that during the reading of that paper by Dr. Detmold, it had occurred to him that, adopting this explanation as correct, we might now perhaps explain a circumstance which had always been a puzzle to chemists and physiologists, viz: the existence in the general atmosphere of from a half to one per cent. of carbonic acid gas. Why this gas should so steadily and uniformly be found, at all heights, and in all places, as a constituent of the atmosphere, had never, he believed, been accounted for, and the only explanation of which it seemed susceptible was that its presence operated in some manner to modify the too great excitement which the oxygen of the air might otherwise produce. The nitrogen with which the oxygen is mixed, is not known to have any such influence—it is a mere *diluent* in this respect, and when the air is absorbed into the lungs, the oxygen is freed from its connection with the nitrogen, and but for the presence of this small proportion of carbonic acid, the great natural as well as artificial anæsthetic, the excitement produced by the action of the oxygen would probably be too great for the smooth and painless workings of the various functions. The accumulation of carbonic acid gas in crowded and unventilated rooms, may on the same principle account for much of the well known soporific influence of such places.

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3. *The Physiological Test for Strychnine.* By MARSHALL HALL, M. D. (London Lancet.)

The chemical tests of strychnia are often found to fail, and as the detection of this poison is a matter of deep public interest, we devote some space to the ingenious experiments of Dr. Marshall Hall, who calls in the aid of physiology to elucidate this difficult subject.

Dr. Hall's experiments prove that if a frog is placed in a solution containing strychnine, it will immediately become violently tetanic, and all the symptoms of poisoning by the alkaloid will be exhibited. In the language of the experimenter, "the frog becomes strychnoscopic." These symptoms occur at once, if the solution contains more than a thirtieth of a grain of strychnia, and continued experiments show that one four hundredth of a grain will produce tetanic spasms in the frog after the lapse of some hours.

If then the contents of the stomach suspected to contain strychnia are collected and a frog immersed in their contents, we are enabled to detect *at once* if there is present more than the thirtieth of a grain of the poison; and as a quantity smaller than this can never produce death in the human subject, we may regard this *physiological test* as adding much to our information on this important subject.

MATERIA MEDICA AND PHARMACY.

1. *Medicated Collodion.* (Translated from the Bull. de Therap.)

It has been recently proposed, originally we believe by M. Hysch of St. Petersburgh, to make the adhesive qualities of collodion useful, by combining it with various external applications, which could thus be retained in close contact with the skin. The first formula of this character was the union of collodion and cantharides prepared by M. Hysch. It answered very well as a revulsive in neuralgias of the face. Dr. Aran, the physician to the Hôpital St Antoine, has recently combined collodion with many substances soluble in æther. Three of these substances may be regarded as forming active and useful preparations. They are corrosive sublimate, iodine and the per chloride of iron.

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The Mercurial Collodion (Hyd. bi. chlor. grs. xxv, collodion ℥ iij,) has been employed as a topical application in the abortive treatment of variolous pustules, with success. Under its influence, the pustules have been prevented from suppurating. As a decisive experiment, M. Aran covered one-half of the face with the mercurial and the other half with the simple collodion. The side protected with the medicated application was free from cicatrices, whilst the other was much marked. It should be applied in the beginning of the disease.

The Iodized Collodion (iodine ʒ j, collodion ℥ iij,) is of a red tint, transparent and elastic. It has been found valuable in chronic herpes and eczema, and has produced excellent effects in the engorgements of the cervix uteri with or without ulceration.

Ferruginous Collodion (ferr. per chlor. ʒ vj, collodion ℥ iij,) is of a tawny color, and forms on the skin a flexible and adherent covering. It possesses much efficacy when applied to erysipelatous and erythematous surfaces, relieving the itching and burning of the parts. It has been applied to the uterine neck, and acts as a powerful constrictor of the relaxed vagina in procidentia.

2. *The Therapeutic Application of Pepsin.*

The memoir of M. Corvisart upon the therapeutic uses of pepsin has been alluded to by us in a former number,* and we notice the verification of his experiments by some of the most eminent practitioners of the French school. M. Barthez records the happy results of its use in many cases of infantile diarrhœa accompanied with indigestion; and M. Rilliet reports similar cases. Debort also confirms these favorable results, and reports three cases of obstinate diarrhœa in nursing children, rapidly cured by the administration of the peptic powder.

We can readily perceive that the natural ferment, under whose influence the albuminous compounds are dissolved in the stomach, ought to favor the digestion of these matters, and that indigestion depending upon a deficiency of this essential element of the gastric juice, ought to be easily cured by the treatment now under notice.

Pepsin may be administered in doses of *three grains* at the beginning of every meal, and the experience of those who have used it justifies us in attaching to it a very considerable importance.—*Journal de Med. de Bordeaux.*

* Va. Med. and Surg. Journal, Vol. V, No. III, p. 226.

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3. *On the Chichike Bark of Guatemala.* By Dr. SCHERZER.

At the session on the 4th of January, of the section on pharmacology of the medical society of Vienna, Doctor Scherzer made some statements concerning various plants, grains and barks of trees, which the natives of Central America use as remedies in certain cases of disease, and which are now but little or not at all known in Europe. For example, the inhabitants of Guatemala use with success in intermittent fever the bark of a tree called chichike, of which Doctor Scherzer presented to the society some of the leaves, flowers and bark. The merit of having first tried the bark, and of having made known its curative virtues, belongs to a physician of Guatemala, Dr. Farfan. The chichike is found in large quantities on the western slope of the Cordilleras, in the state of Guatemala, and does best on slightly moist lands, under a temperature of 80 or 82 degrees Fahrenheit. A quintal of the bark of the chichike costs in the port of Istapa, on the Pacific, but little more than eight piasters, and Doctor S. has engaged to put that quantity at the disposal of the section on pharmacology, for new trials. Considering the high price of the cinchona bark, which increases constantly, the chichike bark, if its efficacy is certain, will be of great importance in therapeutics.—*Am. Med. Monthly.*

4. *On the use of Phosphorus in Ophthalmology.* By Dr. STRUMPF. (Ann. de Therap. 1856.)

The vapors of phosphorus, besides producing general symptoms of intoxication, also cause suppurating ulcers of eye lids. The dead bodies of persons poisoned by this agent have the eyes always deeply sunk in the orbit, and surrounded by large blue circles. The conjunctiva is sometimes red and the cornea always inflamed and the pupils dilated.

In that form of amaurosis occasioned by nervous debility, either general or local, or by a paralysis of the optic nerve or of the retina, appearing suddenly and not the consequence of exterior lesions, when the pupil is still but little dilated and when the symptoms are not acute, inflammatory or febrile, it is in such cases that Lœbenstein-Lobel gives phosphorus in doses of from two to ten drops of a solution of one grain in one drachm of ætherial animal oil, or in the oil of savin, with the addition of one drachm of the ætherial oil

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of cloves or valerian and repeated once during the day. After eight days the remedy should be stopped and such remedies as arnica, or valerian substituted, to which he adds the external use of the dippel oil containing the phosphorus and the ætherial oil of spearmint or the oil of cloves and cajiput with the essence of ants. (*Spiritus formicarum.*) In a case of amaurosis following after menstrual suppression, Hennig of Zerbet prescribed phosphoric æther in doses of 20 drops twice a day.

We have employed phosphorus both internally and externally with success in divers paralyses when the iodide of potass, strychnine, quinine and arnica had failed. Under all circumstances and especially in paralysis of the motor nerves of the eye, this remedy should be used with the greatest caution.—*Jour. de Med. de Bor.*

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *Pneumonia discussed in thirty-three Aphorisms.* By M. Bouchut.

Our readers will be interested in the perusal of the admirably condensed series of aphorisms taken from the excellent work on the Diseases of Nursing Children, by M. Bouchut. This mode of impressing a subject on the memory is very effective and one often employed by the eminent author of this treatise. We are pleased to see that a translation of the work is announced by Dr. Bird of New York, from the publishing house of the Messrs. Wood.

Primary pneumonia, which is also called pneumonia d'emblee, is rare in children at the breast.

Pneumonia usually follows simple bronchitis, or bronchitis complicating fevers, or acute febrile diseases.

Primary pneumonia is usually lobar.

Consecutive pneumonia is always lobular.

Lobular pneumonia is sometimes discrete, sometimes confluent.

The pneumonia of children at the breast is almost always double, and usually attacks both lungs.

Lobar or lobular pneumonia is observed under two anatomical forms, slightly differing as to structure; these are intra-vesicular and extra-vesicular pneumonia.

Intra-vesicular pneumonia, usually primary, leads to congestion and thickening of the walls of the cells of the lung, with the formation of an internal plastic deposit, which constitutes the character of red and gray hepatization.

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Extra-vesicular pneumonia, always consecutive, only produces congestion and thickening of the walls of the pulmonary vesicles, without fibrinous plastic secretion in the interior of these vesicles.

Chronic pneumonia, more common in the infant at the breast than in the adult, is always lobar.

Pneumonia often engenders the formation of fibro-plastic miliary granulations in the interior of the cells of the lung, in lymphatic and scrofulous children, or in the issue of parents tainted with scrofula.

The development of lobular pneumonia is favored by the crowding of children in the wards of a hospital.

Ordinary and frequent cough, accompanied by fever and anhelation, should make us fearful of an invasion of pneumonia.

Expiratory, groaning and jerking respiration is a certain sign of the existence of confluent lobar or lobular pneumonia.

Panting respiration, accompanied by a continual movement of the nostrils, is a sign of pneumonia.

Dullness of the chest is generally but slightly defined in the pneumonia of children at the breast.

When dullness of the chest exists in a young child with a very bad cold, pneumonia should be feared.

Dullness confined to one side of the chest in a young child rather indicates pleurisy than pneumonia.

The subcrepitant rale which accompanies the cough, the fever, and anhelation, confirm the diagnosis of confluent lobular pneumonia.

Bronchial respiration, which is rare in children at the breast, always belongs to lobar pneumonia, and sometimes to confluent lobular pneumonia.

Bronchophony, that is to say, the resounding of the cry, indicates that pneumonia has arrived at its last stage.

The exaggerated vibration of the thoracic walls at the time of the cries, indicates pneumonia, whilst their absence, on the contrary, points out the existence of pleurisy with considerable effusion.

The acute or moderate fever at first continued, presents numerous exacerbations in the course of pneumonia.

Primary pneumonia, or d'emblée, is less severe than consecutive pneumonia.

Pneumonia consecutive to simple pulmonary catarrh is often cured.

Pneumonia consecutive to measles, scarlet fever, small-pox, is a very serious disease.

The pneumonia of children at the breast is, especially, a serious disease, in consequence of the complications which precede or follow its development.

The pneumonia of children at the breast has a great tendency to pass into the chronic state.

The pneumonia which is consecutive to the development of fibro-plastic miliary granulations, or to tubercular granulation, is usually fatal.

Expiratory, groaning and jerking respiration, accompanied by movements of the nostrils, announces that the life of the child is in great danger.

The swelling and œdema of the hands, or of the feet, which comes on in the course of pneumonia, indicates an approaching death. (Trousseau.)

The return of the secretion of tears, which has been suspended in the attack of pneumonia, is a good augury for its favorable termination. (Trousseau.)

One or two leeches at short intervals, several blisters in front of the chest and doses of ipecacuanha, are sufficient for the cure of simple acute pneumonia.

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2. *The Treatment of Itch.* By M. Bourgignon.

The cure of itch, already so rapid, has been still more simplified. After the experiments lately made in the Belgic hospitals by M. Vleminkz, and especially the recent experience of M. Bourgignon, this disease can be always cured in two hours by using the following method:

Take of flowers of sulphur 100 parts, quick lime 200 parts, water 1000 parts. Boil it, and when cold, decant the mixture into bottles, and carefully seal. The treatment consists of, 1st, general friction with strong soap for a half hour; 2nd, simple warm bath for half an hour; 3rd, general friction with the sulphurous mixture over the whole body, permitting the preparation to dry on the skin; 4th, thorough bathing and washing of the whole body will finish the cure.

All this can be done in two hours. It is preferable to the ointment of Helmerich, and leaves a coating of sulphur on the skin, which kills all the vermin which infest the skin. This practice has been followed in Belgium for some years, and is recommended by its simplicity, economy and rapidity of cure.—*Gaz. Hebd.*

In a late number of the *Union Medicale*, M. M. Dusard and Pillon propose a method by which itch may be cured in five minutes. Three drachms of chloride of sulphur are dissolved in three ounces of sulphuret of carbon, and the patient is anointed over the whole body, the patient's face being protected from the sulphurous vapors. In thirty-six hours the application is removed by a bath, when the disease will be found completely eradicated.

SURGICAL PATHOLOGY AND OPERATIONS.

1. *Necrosis of the Inferior Maxillary Bone from the Vapor of Phosphorus.* Removal of the entire Lower Jaw, and Recovery. By Dr. James R. Wood of New York.

The very brilliant operation described in this paper, which we find in the New York Journal of Medicine, May 1856, has many points of interest to the reader. The victim to the deleterious fumes of phosphorus was a "dipper" in a lucifer match factory in New York, an occupation she had followed for two years and a half. The first signs of mis-

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chief appeared in the form of toothache, and one tooth was extracted, but swelling of the jaw-bone supervened, followed by discharge of pus, and she entered Bellevue hospital, and came under the charge of Dr. Wood.

Necrosis of the inferior maxillary bone existed both on the right and left side, and Dr. Wood performed his first operation on the right and most diseased side. Finding the bone necrosed to its articulation, he removed the whole of the right lateral half of the inferior maxillary, and twenty-eight days afterwards he was compelled to perform a like operation on the left lateral half. The patient was discharged on the 20th of March 1856, with remarkable preservation of the contour and comeliness of the face.

Necrosis, the result of the fumes of phosphorus, although a subject of but recent investigation, is not a very rare affection, as in the paper before us, we find *seven* other instances of like character reported by Dr. Van Buren. They have all resulted from the exposure to the fumes of phosphorus in "match factories," and the vast consumption of this modern convenience has undoubtedly developed this sad disease. Dr. Wood remarks:

Phosphorus disease, or necrosis from exposure to the fumes of phosphorus in the manufacture of lucifer matches, was first noticed in Germany. Lorinser of Vienna, published the first account of this disease in 1845, and reported a number of cases. Soon after, Heyfelder of Erlangen, and Strohl of Strasburg, published cases; and in 1847, Drs. Von Bibra and Geist, published a separate work. In the following year, accounts of the disease were published in England; and in noticing a case, in the surgical reports of Guy's hospital (1846–47) of separation and exfoliation of the lower jaw, from exposure to phosphorus, in the manufacture of lucifer matches, it is stated, that the disease was previously noticed to be not uncommon in those working in phosphorus. Mr. Stanley alludes to this disease in his treatise on Diseases of Bones. Cases have been occasionally reported in English periodicals; and in the Lancet for 1850, (vol. i, p. 41,) there is an interesting clinical lecture by Mr. Simon, on this subject, with the full details of a case. Phosphorus disease does not seem to have been frequently noticed in this country, if we may judge by reported cases; yet the causes exist among us in all their intensity. I am aware, indeed, of but a single case which has been placed on record, and that was observed by Dr. Bigelow of Boston. That this disease is more prevalent in this country, than might be inferred from this single case, is evident from the several cases appended to this paper, which I have been able to collect, and the case kindly communicated by Dr. Van Buren.

That phosphorus is the destructive agent in this disease, has been proved by experiments upon animals. Rabbits exposed to the fumes of phosphorus, under circumstances similar to those which determine the disease in man, are similarly affected. Another fact seems clearly established, viz: the vapor of phosphorus must come into immediate contact with the periosteum or bone, in order to excite the morbid process. This explains, in the first place, why but few, comparatively, are

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affected who work in these manufactories; and, in the second place, why the lower jaw is more frequently the seat of the disease than any other bone. For it appears that those only suffer who have decayed teeth—the defect in the teeth allowing the fumes of phosphorus to penetrate to the periosteum. So important is this latter fact, that the government of Erfurt has passed a decree, that no person having decayed teeth shall be allowed to work in lucifer match factories. In a factory in this city, no workman is allowed to return to his work for a week after the extraction of a tooth.

The prognosis in these cases is very unfavorable. When the disease first comes under notice, the periosteal inflammation has generally long existed, and new formations already separate the bone from its covering. More frequently the suppuration is established, exfoliations of bone are taking place, and the whole morbid process is in active progress. The system now breaks down under the exhausting discharges and poisonous emanations from the jaw; and the miserable subject of this destructive disease falls a victim to its inroads upon his strength long before the completion of the process of exfoliation.

2. *Dressing of Stumps Under Water.* By MM. LANGENBECK & VALETTE.

The difficulty of getting healthy stumps, after amputations performed in the large European hospitals, has led to many original and ingenious experiments in dressing. As one of the most recent, we notice the following suggestion in the Lancet:

M. Langenbeck of Berlin has, for some time past, been in the habit of placing stumps in zinc boxes filled with warm water; and it is stated that some of the unpleasant sequelæ of extensive wounds are, by this kind of dressing under water, completely avoided. M. Valette, surgeon to the Charité hospital of Lyons, considers that this method owes its success to the fact that the wound is protected from the action of atmospheric air, the analogy being drawn from the phenomena attending subcutaneous solutions of continuity. M. Valette leaves the stump a fortnight or twenty days under cold water, or an infusion of aromatic plants in which some alum is dissolved. Pus is, in this manner, coagulated and thrown down. He thinks that the present method is calculated to prevent purulent absorption; symptomatic fever is much slighter than by the ordinary method of dressing, and the pain is considerably less. M. Langenbeck is on the point of using caoutchouc bags instead of zinc boxes; the bags are connected with two tubes, one bringing the water from a pail placed on a shaft, the other acting as a waste pipe. A continuous current may be established at will, and keeps the part perfectly free from any accumulated secretions. This latter method is certainly worth trying.

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BIBLIOGRAPHICAL RECORD.

I. *Report of the Committee upon Obstetrics, to the Medical and Chirurgical Faculty of Maryland.* June 1855. Committee, Drs. W. Chew, Van Bibber, C. O'Donovan and Gerard Morgan.

This report is one of a number of valuable papers, for which the profession is indebted to the zeal and industry of our brethren of Maryland. The "Medical and Chirurgical Faculty" of that state corresponds to the medical societies of other states. Among the proceedings of its annual meetings is the presentation of reports by committees appointed at the preceding meeting, upon different departments of medical science.

The report of which the title is given above, derives a peculiar value from the nature of the materials on which it is based. It contains a statistical summary, with illustrative cases, of the large obstetrical experience of the late Dr. Peter Chatard. This gentleman, who was a native of France, and emigrated to this country after commencing the practice of his profession in one of the West India islands, commanded for a long series of years, (more than half a century, we believe,) the confidence of the people of Baltimore, as a skillful and upright physician. Few men in this country have enjoyed so large an obstetrical experience, and still fewer have taken equal pains to note its results in a durable form. He left a record, in his native tongue, of 4,309 cases of parturition, of which 1,787 are recorded accurately and in full detail. The remaining 2,522 are summed up in a statistical form from previous records. These valuable documents have been carefully analysed by Dr. Van Bibber, who in the report before us has set forth the leading results deducible from them, in numerical statements. These statements refer to the *number of children* the patients had

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severally borne, the *duration of the labor*, the relative frequency of the different *presentations*, the proportion of *male and female children*, the proportion of *births occurring in each month* of the year for the two sexes together, and for each sex separately, the *weight* of the children, as ascertained in 811 cases, the number of children *still-born*, the number of *abortions and premature births*, of *deformed children*, of *twin births*, of cases of *flooding*, of *convulsions*, of *placenta prævia*, of *adherent placenta*, the number of *instrumental deliveries*, &c.

The information given under these different heads furnishes many points of interest; but it would be impossible to transfer it to our pages without reproducing almost the entire report, for this is itself but an abstract. The results, however, do not differ materially (for the most part) from those of similar statistical records already in possession of the science. The most striking—perhaps we should say *surprising statement*—refers to the number of *instrumental deliveries*. In 4,290 labors, the *forceps* was employed in 144 cases, or *once in every* 30 *cases*, (nearly;) and craniotomy was performed seven times, or once in 613 cases. "Commenting upon his book of practice after thirty years, Dr. Chatard says, 'as to the applications of the forceps, they are evidently very numerous. The use of this instrument might have been reduced by three-fourths, but feeling certain of not injuring the children, and desirous of abridging the sufferings of the mothers, I did not hesitate to make use of it.'"

Truly, these numbers authorize us to characterize Dr. Chatard's as *heroic practice*. The employment of the forceps in one labor out of 30 is rather shocking to the weak nerves of those timid people who believe the forceps to be "a formidable weapon," capable of inflicting fearful injury, not so much on the child, (which would seem from the above quotation to have claimed Dr. C.'s chief consideration,) as on the mother. It was fortunate indeed for the ladies of Baltimore, that a practitioner so bold and impatient, was at the same time so skillful in the use of this powerful instrument. His example would be a very unsafe one for less practiced

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and less cunning hands to follow. That so frequent a resort to the forceps is quite unnecessary, (as Dr. Chatard indeed admits,) and is therefore wrong, is evident from the fact that the highest obstetrical authorities assure us that they have not found it necessary to employ this instrument in more than one case in two or three hundred. This is true even in France, where the fear of instruments is certainly not a feature of obstetrical practice: while in Dublin, where all unnecessary meddling is religiously eschewed, only one woman in six or seven hundred is delivered with the forceps.

Among the cases which are given in the report, in greater or less detail, the following seem worthy of note:

Case xiv is that of a woman who gave birth to a child *at the age of* 50 *years.*

Case xviii.—"This lady carried her child 315 days, that is *ten months and a half*, or nearly so, counting from the time of feeling the first motion of the child, and from the suppression of the catamenia. Her calculation was certainly as exact as possible, and I do not doubt that the gestation was prolonged to that term."

Case xxvi.—"Mrs. D. primapara, after a pregnancy of *four months*, gave birth to a fœtus which had only the ordinary development of that period of pregnancy. Nevertheless, *it lived* 29 *hours*, cried, passed meconium, and took sugar and water several times. The child was not weighed, but I am sure it would not have weighed more than 10 ounces." If there was no mistake as to the age of the fœtus, this case is probably unparalleled.

Case xxviii is that of a lady, who at the period of five months and a half was seized with pains like those of labor, and lost a great quantity of water. Nevertheless, both the pains and the discharge of water ceased, and she reached the term of nine months, when she was delivered of a large and healthy child.

"Another case equally rare, but of a different kind, took place in another woman at the same period of her pregnancy; instead of a loss of *water*, it was a *torrent of blood.*

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The quantity was very great, and finding that the loss of blood was arrested, and firmly persuaded that the miscarriage had taken place, I left the woman without making any investigation, relying on the report of the nurse. I was sent for at the full term of nine months, to remedy an unknown difficulty in the same woman. I found that the membranes were so thick and strong that it was impossible for the head of the child to make its way through; but after having incised them, the womb disburdened itself of a large boy full of life."

Case xxxvii.—"I was sent for in consultation for Mrs. H. whom I delivered of a very large placenta, after 15 days of suffering. She thought herself in the third month of pregnancy, but I could discover *no fœtal conception.*

L. S. J.

II. *Atlas of Cutaneous Diseases.* By J. Moore Neligan, M. D. Edinburgh. Philadelphia. 1856. Blanchard & Lea. (Received from G. M. West.)

The profession owes its thanks to the publishers of Neligan's Atlas of Cutaneous Diseases, for they have placed within its reach and at a moderate cost a most accurate and well delineated series of plates illustrating the eruptive disorders. These plates are all drawn from the life, and in many of them the daguerreotype has been employed with great success.

Such works as these are especially useful to country practitioners, who have not an opportunity of seeing the rarer forms of cutaneous disease, and hence need the aid of illustrations to give them the requisite information on the subject. Fortunately for the community, these mutilating and malignant affections are not often seen in our country; but when they do occur, it is of great importance to the physician that he should be able to diagnose accurately, and prescribe correctly.

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With these plates at hand, the inexperienced practitioner is enabled to discriminate with much accuracy, and he is thus, comparatively speaking, put on an equal footing with those who have had the opportunity of visiting the large hospitals of Europe and America.

We are disposed to object occasionally to the high coloring of some of the lithographs for this work, but it is often necessary to exaggerate to a certain extent such illustrations, in order to produce the proper effect, and besides it is of course best to select the more strongly marked cases as typical of the class of disease to which they belong. We advise our readers to add this book to their libraries.

III. *The Principles of Surgery.* By JAMES MILLER, F. R. S. E. Fourth American from the Third English Edition. (Received from A. Morris.)

THE book before us has been stamped with the approbation of both the English and American public, and requires us to say but little in its behalf. Mr. Miller has achieved a character for judgment and discretion, combined with great frankness and willingness to confess error, which makes his book very reliable and in every way worthy of confidence.

No similar production has been so much used as a text book for medical students, and we doubt whether there is another to be found better adapted for elementary instruction in the principles of surgery. The author, in his third edition, makes many alterations of text, the fruits of his enlarged experience and sounder views, and we anticipate for the fourth American edition a rapid sale.

IV. *On some Diseases of Women, admitting of Surgical Treatment.* By ISAAC BAKER BROWN. Philadelphia. Blanchard & Lea. 1856. (Received from J. W. Randolph.)

MANY of our readers have received this work in the Medical News and Library, and the publishers have now offered it to

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the public in book form. The subjects of which it treats lie intermediate to the domains of obstetrics and surgery, and are often passed over or dismissed with a cursory remark in the general treatises on these branches.

The author divides his subject into those surgical affections, the result of pregnancy or delivery, and that class occurring independently of pregnancy.

Under the first section are classed operations for 1, rupture of the perineum; 2, prolapsus vaginæ; 3, prolapsus and procidentia uteri; 4, vesico-vaginal fistula; 5, recto-vaginal fistula; and 6, lacerated vagina.

Under the second section are classed operations for 1, polypus uteri; 2, stone in the female bladder; 3, vascular tumor of the meatus urinarius; 4, imperforate hymen; 5, encysted tumors of the labia; 6, diseases of the rectum resulting from certain conditions of the uterus; and 7, ovarian tumors.

We notice that in the treatment of vesico and recto-vaginal fistulæ, Mr. Brown alludes with commendation to the method of Dr. Simms, known as the clamp suture, and indeed throughout the whole work we see marks of careful record and accurate information.

EDITORIAL AND MISCELLANEOUS.

The American Medical Association.

The friends of our national organization may congratulate themselves on the late meeting at Detroit, which was emphatically a working session. No previous meeting has displayed greater fidelity on the part of reporters of committees, nor more diligence in the despatch of business by the association itself. Consequently, though the attendance of delegates was small, on account of the remoteness of the

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place of meeting, a large and well filled volume of Transactions may be expected as the result of their labors.

Our readers will find a detailed account of the proceedings in another part of this journal. They will be gratified to observe the harmony which prevailed, and the earnest desire to promote the interests of the profession, which was generally manifested. We do not suppose that the thorough-paced reformers will be satisfied, for the hundred or more gentlemen at Detroit did not devise any plan for the immediate redress of the grievances of their fifty thousand brethren in these states, nor even undertake to correct the professional abuses we all lament, by any euphonious series of resolutions. But dwellers out of Utopia will be glad to see original researches promoted, organized effort encouraged, and medical instructors advised by the best men in our ranks on feasible plans for elevating the standard of medical education.

The various constitutional amendments of last year were rejected at Detroit, and a batch of new ones were proposed, for which we may predict a similar fate. There seems, however, to be a growing inclination to give the association a local habitation, and to meet permanently in some central locality, at least on alternate years. Although the constitutional amendment, embodying Dr. Drake's favorite project of connecting the association with the Smithsonian institution, was not adopted, yet a committee was appointed to confer with the managers of that institution, and we are inclined to believe that this measure will ultimately result in locating at Washington the archives of the association, and its biennial sessions.

Only four of the southern states were represented at the meeting by delegates, though they sent on their full quota of communications. Inasmuch as there was no one to supply the committee on nominations with information in regard to appointments of special reporters in this section, but few were appointed.

Quite an animated discussion arose during the meeting on

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the antagonistic views of the regular report on medical literature and the special report on the same subject by Dr. Gross. The latter strenuously advocated the more general adoption of the works of American authors as text books. This would be more practicable, if our literature could boast of more names like that of the accomplished reporter, whose pathological anatomy was the best in the language, at the time it was published, by the admission of the Medico-Chirurgical Review even, whose treatises on the urinary organs and air passages occupy a permanent place in literature, and whose researches on wounds of the intestines are as original as they are valuable; but there are many subjects in medicine on which there are as yet no systematic American treatises, and we cannot but think that Professor Gross has begun at the wrong end in his suggestions for improving our national literature. In the conclusion of his report, Dr. Gross exhorts medical journalists to be more just to the writings of their compatriots. Now we know of no medical periodical in the country which does not welcome the communications of American physicians, and the criticisms of American books are usually in a strain of exaggerated eulogy. We do not comprehend this fling at the journalists. We shall revert to this subject, merely observing now, that it will be time enough to raise the cry of illiberality and injustice when journalists reject the contributions of native writers in favor of foreign productions. The beamy eyes of members of the association who detect the motes in the organs of teachers and journalists with such cruel perspicacity, are blind enough to their own shortcomings. We know of physicians who clamored loudly for long sessions at the medical schools, and an improved standard of requirement—who, when the university of Pennsylvania, and the New York college of physicians adopted their suggestions, sent their students to the rival institutions which adhered to the old standard, and thus obtained a numerical superiority over these brave old schools. And we know, too, of physicians who grumble at the periodicals, who never write any thing,

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nor read any thing, nor pay any thing, nor assist in any other way in supporting and promoting the medical literature of the country.

Another discussion, of a personal character, was excited by the presentation of a morbid specimen, by Dr. Wood of New York. Dr. Neill of Philadelphia considered (justly, we think,) that this practice was liable to abuse, and moved that individual cases should not be brought before the association until they had first been submitted to a special committee. Dr. Wood rejoined in a manly disclaimer of any desire to court notoriety, and, on his proposition, the notice of his operation was stricken from the minutes.

The names of *McClintock*, formerly a professor in Philadelphia, who sold himself some time since to nostrum venders, and *Cleaveland*, who, in a more specious manner, has allied himself with quackery in the so called eclectic college at Cincinnati, were expunged from the roll.

A tribute of respect was paid to the memory of our lamented friend and predecessor, Gooch, by a series of resolutions, introduced by a special committee. We shall publish a copy of these resolutions in our next issue.

Next year the association meets at a point which is comparatively accessible to the majority of our readers. We sincerely hope that a large number of them will make their arrangements to be present on that occasion.

Medical Society of Virginia.

We regret to announce that the members of the state medical society, who met in Richmond on the 22nd of April, were too few in number to constitute a quorum; and that, consequently, there was no annual meeting. We would respectfully suggest that the executive committee should meet and devise some measures to revive, if possible, an interest in this association.

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Dr. John C. Warren.

THIS distinguished surgeon terminated his long and useful career on May 4th, at his residence in Boston. His death, we are told by his "renowned coeval," the venerable Dr. James Jackson, could not be attributed to any distinct disease. This opinion of his sagacious attendant was corroborated by the autopsy, which revealed no local lesion of importance.

Warren was a man of indefatigable industry. He has written on many subjects with unquestionable ability—though, as a medical author, he has produced no masterpiece—and he has engaged in active professional labor for half a century. It has been remarked that great operating surgeons are often like great actors or great musicians—heroes of the moment. The wonderful tact, the natural or acquired intuition by which they obtain their glory, dies with them. This may be true; but Warren had other claims on the admiration of his compatriots, which will preserve his memory from oblivion. His desire for knowledge was not limited by the boundaries of his profession, and he instructed two generations of medical men in the principles of their art.

Dr. Warren was usually considered a cold man, and this was our opinion of him. But Dr. Jackson, who knew him best, testifies that his feelings were deep and strong, though unaccompanied by outward emotion. In another part of the journal some of the incidents of his life and death will be found recorded.

Volume Sixth.

THE present number concludes the sixth volume of the *Stethoscope* and of the *Virginia Medical and Surgical Journal*, and the first volume of the combined journals. We take occasion to thank our contributors for their valued communica-

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tions, and to solicit a continuance of their kind collaboration. We are happy to be able to state that the subscription list has largely and steadily increased since January last. We hope, however, that our friends will not discontinue their efforts to augment the circulation of our work. Additional receipts will enable us to make still larger expenditures in illustrating and improving the journal. By reference to the index, the reader will perceive that this volume consists of *five hundred and fifty* pages of reading matter, with eight extended tables, and numerous illustrations. The greater part of the reading matter is original, or prepared from original sources. We hope to improve on this in the next volume, with the assistance of our correspondents, whom we would particularly exhort to send us records of cases, and brief practical papers. We are like Mr. Gradgrind, and have an insatiable appetite for *facts.*

Varia.

Boileau said of La Bruyère, the delightful author of *Les Caractères*, that he often avoided the most difficult part of composition, that is the transitions. In the lucubrations which we have tacked together for our *miscellany* for this month, we shall imitate, at least in that respect, this illustrious example.

* * *

It is an infallible sign that a science is unsettled, when it is regarded as a sort of common property. My boot black does not hesitate to define diseases, to assign their causes, to prescribe remedies, and predict results. He thinks himself competent to do this, and his comrades sanction his opinion; they listen to his advice, and often follow it. This position of medicine is that which it had two thousand years ago.

* * *

Vita brevis, ars longa, experientia fallax, judicium difficile. This first line of medical science is very fine, but it is not encouraging. We wonder how Hippocrates, after writing aphorism number one, could write a second. Perhaps the master of masters placed it at the head of the others to serve as their passport and apology. He wished to teach us that we must not accept his maxims lightly, or make others ourselves without cautious reflection. He gives us a lesson of modesty and of

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prudence at the same time. It would seem as though we had never comprehended either the example or the precept. It would seem as though the medical mind had continually oscillated between a rash dogmatism and a barren scepticism. Medicine requires some such *criticism* as that to which psychology has been subjected by Kant. It would lose many of its ambitious pretensions by this cruel though salutary operation, and many rights it has usurped, but its affairs would be set in order, and it could enjoy with security and honor its narrrow but uncontested domain. Hitherto, medicine, science, art, and profession, has had only apologists or detractors, advocates or enemies, special pleaders on both sides. There has been no judge. Where is the Kant that can arrange our business? Will not our friends, the *Reformers*, point out the man? Wherever he is, let him show himself, for his time has come.

* * *

These antagonistic tendencies of medicine, dogmatism and scepticism, are alike baneful, though not in the same manner or degree. The practical consequences are what we dread in the first, for dogmatism impels to the *faciamus experimentum*, which is a serious matter when the question is, as Baglivi says, *de pelli humana*. Scepticism, leading to inaction, is commonly not positively offensive; but it is the death of science and art—an intellectual suicide. These two vices usually alternate in the history of medicine; when one prevails, the other is repressed, until, through the excesses of its opponent, it regains the upper hand.

Scepticism may reign *incognito*, so to speak. Physicians of this way of thinking do not advertise their opinions. They are generally quiet and treacherous, whereas the dogmatists are argumentative and noisy. Medicine—why dissimulate it?—is the chosen seat of scepticism. This vice, an accident in other sciences, is a natural product in our own. The majority of the systematic unbelievers and contemners of medicine, who have attracted attention, have been physicians. Sextus Empiricus, Cornelius Agrippa, Leonardo of Capua, were physicians; and it was a physician, the learned Sprengel, the historian of medicine, who, a little while ago, published, under the name of Arcesilas, a formal denunciation against the science to which the labors of his whole life had been devoted. Let us note, however, that his scepticism, like that of other malcontents, was not universal. He had still respect for one little idol, the doctrine of *excitement*, the Brunonianism which for the hour was the divinity in vogue. So did before him, or have done since his time, those fierce reformers, Themison, Galen, Paracelsus, Stahl, Cullen, Rasori, Brown and Broussais, who would leave nothing standing in sciences—except themselves. However it may be in regard to these contradictions, *quas parum humana cavet natura*, it is indisputably true that the most murderous assaults on medicine have ever proceeded from her own disciples. It is not a simple result of chance that so many

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names of physicians appear on the catalogue of sceptical philosophers. There are not less than six among those of antiquity whose works have come down to us, and the earliest promoters of modern pyrrhonism were the physician C. Agrippa, with his libel *de incertitudine et vanitate scientarum*, the physician Sanchez, with his famous *quod nihil scitur*, and the Spanish physician Martinez, with his *philosophia sceptica*. These are not fortuitous coincidences; they indicate too clearly the relation between sceptical tendencies and the study and practice of medicine.

* * *

In addition to the attacks of the false brethren, medicine has been assailed by the injurious criticisms of wits, such as Pliny and Voltaire, and by the epigrams and satires of poets and romancers. The wounds inflicted by these adversaries have never penetrated beyond the epidermis, and we can afford to make light of them. Among these pleasantries, there is one I will repeat, because it is good. It is in dog latin, and deserves no better:

Si vis sanari de morbo nescio quali
Accipias herbam, sed quam vel nescio qualem
Ponas nescio quo, sanabere nescio quando.

American Medical Association. Proceedings of the Ninth Annual Meeting, held in Detroit, Michigan, May 6th to 9th, 1856.

On Tuesday morning, May 6th, the association convened in Firemen's hall, and was called to order at 11 o'clock by the president, Professor GEORGE B. WOOD of Philadelphia.

Dr. ZINA PITCHER of Michigan bade the members of the association welcome.

The junior secretary, Dr. WILLIAM BRODIE of Detroit, took his seat, and called the roll.

On motion of Dr. THOMPSON of Delaware, the nominating committee was constituted as usual, each state delegation selecting one member to serve in this capacity.

Drs. N. P. Monroe of Maine, H. Pierce of New Hampshire, C. L. Allen of Vermont, H. H. Childs of Massachusetts, J. E. Warren of Rhode Island, David Harrison of Connecticut, William Rockwell of New York, L. A. Smith of New Jersey, John Neill of Pennsylvania, J. W. Thompson of Delaware, P. Wroth of Maryland, E. Geddings of South Carolina, J. B. Lindsley of Tennessee, W. S. Sutton of Kentucky, C. W. Le Boutillier of Minnesota, M. Gunn of Michigan, Thomas W. Gordon of Ohio, Dr. Winton of Indiana, H. Noble of Illinois, W. H. Brisbane of Wisconsin.

When the nominating committee retired, Dr. PITCHER, chairman of the committee of arrangements, suggested that the association should hold two sessions daily, from 9 to 12½ and from 2 to 5, except on Wednesday, when there should be a morning session only.

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The PRESIDENT announced the death of Dr. JOHN C. WARREN of Boston. Dr. CHILDS of Massachusetts, and Dr. GROSS of Kentucky spoke in eulogy of the eminent deceased. A committee was then appointed to draft resolutions of respect for the memory of Dr. Warren.

The association then adjourned to 2 P. M.

The attendance was rather thin in comparison with some previous meetings, but yet there were over an hundred delegates from a distance, who had sought the northern frontier, to honor the hospitable invitation of our brethren in Detroit.

Afternoon Session.

The secretary read letters from the Tennessee medical society and from the university of Nashville, inviting the association to hold its next meeting in Nashville; also a letter tendering the use of the Tennessee hall of representatives for that session; all of which were referred to the nominating committee.

The nominating committee recommended the following officers, who were elected:

President—Dr. ZINA PITCHER of Detroit.
Vice-Presidents—Drs. THOMAS W. BLATCHFORD of New York, WILLIAM K. BOWLING of Tennessee, E. GEDDINGS of South Carolina, W. H. BRISBANE of Wisconsin.
Secretaries—Drs. WILLIAM BRODIE of Michigan, R. C. FOSTER of Tennessee.
Treasurer—Dr. CASPAR WISTAR of Pennsylvania.

The PRESIDENT then delivered the annual address.

The retiring president, Dr. WOOD, appointed a committee to conduct the president elect to the chair. Upon taking the chair, Dr. PITCHER made his acknowledgments in a neat speech.

On motion of Dr. GUNN of Michigan,

Resolved, that the resolution passed at St. Louis, requiring a majority of the committee on publication to be appointed from residents of the place where the meeting is held, be repealed.

The committee on prize essays reported through its chairman, Dr. A. B. PALMER of Illinois. The committee had received only four communications. Each of these was meritorious, but the one bearing the motto, "*Una est veritas*" alone possessed that "wide scope and rigid accuracy of logical reasoning, that chasteness of expression," etc. which were necessary to furnish a claim for the award of a prize. The successful essay treated *of the arterial circulation*, and Dr. HENRY HARTSHORN of Philadelphia was found to be its author, and the laureat of the present year.

Dr. BLATCHFORD of New York read a report on hydrophobia, and the connection of the season of the year with its prevalence; which had evidently been prepared with much care and after extensive reading and investigation. It concluded with the following resolution, which was adopted:

Resolved, that the secretary transmit to the governor of each state the statistical part of this report, with a request to bring the subject before the legislature, in order to devise some plan to mitigate if not remove this disease.

Dr. WISTAR of Philadelphia read the report of the committee of publication.

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It stated that the first copies of the *Transactions* for 1855 were issued in November; that 1,100 copies were printed; that the aggregate expense of printing, illustrating and binding was $1,922 70; that the distribution of the volume was effected, in every possible instance, by express; that Drs. C. Hooker of Connecticut, Alden March of Albany, J. L. Atlee of Pennsylvania, W. Brodie of Michigan, C. B. Gibson of Richmond, E. L. Beadle of New York, H. W. Dessaussure of South Carolina, C. A. Pope of Missouri, D. H. Storer of Massachusetts, T. H. Richardson of Kentucky, J. Mauran of Rhode Island, T. Miller of District of Columbia, F. E. B. Hintze of Maryland, L. P. Bush of Delaware, Z. Pitcher of Michigan, and J. B. Lindsley of Tennessee, have rendered essential service to the association—some in procuring subscriptions to the volume, and all by cordial co-operation in its distribution; that it is important to secure the efficient co-operation in every state by the appointment of gentlemen whose duty it shall be to aid in procuring subscriptions for and circulating the Transactions; that Connecticut is especially to be commended for her services in this particular; that not a little embarrassment was experienced by the committee in restoring to the list of permanent members the names of those who had been left off by order of the association for nonpayment of assessments; that they had endeavored, however, by careful comparison of the various lists, to supply all omissions; that the committee had been reluctantly obliged to omit from the Transactions two valuable reports on epidemic diseases—by Dr. Anderson of Alabama, and Dr. E. D. Fenner of New Orleans—but, as they had not been presented to the association, and acted on by that body, there was no other alternative; that the following resolution, passed at the last session, should be strictly enforced:

Resolved, that hereafter, beginning with the session of 1856, no report or other paper shall be entitled to publication in the volume for the year in which it shall be presented to the association, unless it be placed in the hands of the committee of publication on or before the 1st of June.

The report further states that the number of volumes of Transactions now remaining on hand is as follows: Of Vol. I, 41; of Vol. II, 9; of Vol. III, 32; of Vol. IV, 7; of Vol. V, 316; of Vol. VI, 66; of Vol. VII, 120; of Vol. VIII, 351. That some of the leading journals abroad have expressed a strong desire to complete their sets; and it rests with the association to determine whether the missing numbers shall be supplied. That, as only seven complete sets of the Transactions are now in the possession of the association, the committee recommend that no copy of either of the eight volumes which is necessary to the complete sets now remaining, shall be disposed of separately, or with any number of volumes short of a complete set.

After some remarks by Dr. ATLEE, in the course of which he observed that the Smithsonian institution had been offered as a permanent place of meeting for the association, the report was accepted, and it was ordered that the committee on publication should reserve five complete sets of the Transactions.

Dr. WISTAR then read, as treasurer, the annual report on the financial condition of the association. The chief disbursements had been on account of the eighth volume of Transactions; for engraving, $72; for lithographing, $101; for printing and binding, $1,748. The assessments and sales had yielded $2,468.

Dr. MCNULTY of the New York academy of medicine, proposed that a committee should be appointed to address the people of the United States on the claims of the medical profession to their gratitude and confidence. After some debate, this absurd proposition was withdrawn.

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A member stated that another member, then present, had lately performed an extraordinary operation—the removal of a jaw for necrosis resulting from lucifer match maker's disease, and moved that a time be appointed for the association to examine the pathological specimen. Dr. JAMES WOOD of New York, the operator referred to, said that he would lay the specimen on the president's desk in the morning.

The PRESIDENT read a communication from Dr. STEWART, chairman of the committee appointed last year to consider the propriety of extending the lectures of each chair in medical schools over a period of two years, stating that the views of medical instructors had as yet been imperfectly ascertained.

The association then adjourned.

In the evening, the members were received by Dr. H. P. COBB, Dr. M. STEWART, E. A. BRUSH, Esq. and ALBERT CRANE, Esq.

Second Day, May 7th, 1856.

The association met at 9 A. M. The minutes of the preceding day were read and approved. The secretary then read letters from Drs. Semmes, Bradford, Reese, Peaslee, Corson, Stevenson, Beech and Hutchinson, chairmen of various special committees, asking for farther indulgence in order to complete their reports. These communications were referred to the committee on nominations.

The secretary announced that he had received a resolution adopted at the last meeting of the New York state medical society, inviting members of the association to meet with the society at its semicentennial celebration, on the first Tuesday of February 1857.

The committee on nominations reported in favor of holding the next meeting at Nashville, Tennessee, and their recommendation was agreed to.

The secretary read the following communication from the secretary of the Ohio state medical society:

SIR—At the annual meeting of this society, held in June last, at Zanesville, Ohio, the following resolutions were adopted, and I was directed to furnish you with a copy of the same:

Resolved, that the resolution offered by Dr. GRANT (a member of this society, but not at this or at that time a practitioner of medicine, but a lawyer) at the last session of this society, viz: "That it is not derogatory to medical dignity, or inconsistent with medical honor, for medical gentlemen to take out a patent right for surgical or mechanical instruments," was offered at a time when many members had left for their homes, and is not, therefore, the sense of the society.

Resolved, that the said resolution is in direct opposition to the code of medical ethics adopted by this society; and, therefore, be it further

Resolved, that said resolution, offered by Dr. GRANT, and adopted by the society, be and is hereby rescinded, and placed upon the minutes.

The secretary read a communication from Dr. Hamilton of Buffalo, asking leave to incorporate the report on deformities after fracture in a work on fractures and dislocations he is now preparing for publication. Dr. ATLEE urged that the request might be granted, which was objected to by Dr. LINDSLEY, on the ground that a similar petition had been refused at the St. Louis meeting. On motion of Dr. PALMER, the matter was finally referred to a select committee consisting of Drs. Palmer, Atlee and Hills.

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On motion of Dr. J. B. LINDSLEY of Tennessee, it was

Resolved, that a committee of three be appointed by the chair to prepare a suitable minute in reference to the death of our late secretary, Dr. P. C. Gooch of Richmond, Va. who fell a martyr while contending with the pestilence in Norfolk in 1855.

The PRESIDENT appointed Drs. Lindsley of Tennessee, Thompson of Delaware and Mendenhall of Ohio to carry out the object of the resolution.

Dr. SUTTON of Kentucky moved a vote of thanks to the late president, Dr. WOOD, for his address, and proposed that a thousand copies of it should be printed; which was agreed to.

Resolutions relating to the death of Dr. Warren were reported by the committee appointed yesterday, approved, and placed on file.

Professor GROSS read a report on the causes which impede the progress of American medical literature. He concluded by offering the following resolutions:

Resolved, that this association earnestly and respectfully recommends: 1. The universal adoption, whenever practicable by our schools, of American works, as text books for their pupils. 2. The discontinuance of the practice of editing foreign writings. 3. A more independent course of the medical periodical press towards foreign productions, and a more liberal one towards American. And, 4. A better and more efficient employment of the facts which are continually furnished by our public institutions, for the elucidation of the nature of diseases and accidents, and, indirectly, for the formation of an original, a vigorous, and an independent national medical literature.

Resolved, that we venerate the writings of the great medical men, past and present, of our country, and that we consider them as an important element of our national medical literature.

Resolved, that we shall always hail with pleasure any useful or valuable work emanating from the European press, and that we shall always extend to them a cordial welcome, as books of reference, to acquaint us with the progress of legitimate medicine abroad, and to enlighten us in regard to any new facts of which they may be the repositories.

Dr. PHELPS of New York moved the adoption of the report and resolutions, as a whole. It was, however, decided to vote separately, and, after the acceptance of the report, the first resolution was read, when a member proposed to substitute the word "just" for "liberal," in the clause referring to the course of medical periodicals in relation to native productions. Dr. GROSS accepted this amendment.

Dr. PALMER asked in what sense the expression "practicable" was employed. If it was meant that inferior works by American authors should be used in medical schools in preference to better treatises of foreign origin, he (Dr. PALMER) was opposed to the resolution. Of course, an American work should have the preference over a foreign one of equal merit. Dr. PALMER then alluded to some systematic works by eminent English and French writers, for which there were no American substitutes.

Dr. GROSS admitted that one of the works adduced by Dr. PALMER must of necessity be placed in the hands of our medical students, as there was no American book on the same subject. As a general rule, foreign publications should be used for reference, and American works, "when practicable," as text books.

Dr. COBB of New York opposed the resolution in a brief speech, which made somewhat of a sensation. He said that the resolutions *savored too much of know-nothingism* to make them palatable.

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Dr. LEIDY of Philadelphia was in favor of allowing teachers to select their own text books.

On motion of Dr. DAVIS, the resolutions were laid upon the table.

Dr. BRECKENRIDGE of Kentucky then read a report on American medical literature.

On motion of Dr. HOOKER, the report was accepted, and referred to the committee on publication.

The secretary read a communication from Dr. P. A. Jewett of Connecticut, chairman of the committee to procure memoirs of the eminent and worthy dead. Referred to committee on nominations.

Dr. DARNALL of Indiana moved that special committees should be appointed to report on the following subjects: "The value of a strict attention to position in the treatment of diseases of the abdomen," and "The value of the external application of cold in the treatment of diseases of the chest and abdomen." Referred to committee on nominations.

The association then adjourned.

The afternoon was devoted to a grand frolic. The delegates (happy dogs) with "two hundred ladies," embarked, with their hospitable entertainers, on the steamer *Western World*, and had a glorious sail over Lake St. Clair. There were music and dancing and a sumptuous dinner.

Third Day, May 8th, 1856.

The association met at the usual hour.

Dr. ATLEE offered the following resolution, which was adopted:

Resolved, that the president shall be authorized annually to appoint delegates to represent this association at the meetings of the British association, the American medical society of Paris, and such other scientific bodies in Europe as may be affiliated with us. Adopted.

Dr. CLENDENNIN offered the following:

Resolved, that a committee of one be appointed for a period of three years, with instructions to report progress at each annual meeting of the society, to investigate the etiology and pathology of epidemic cholera, and that said committee be allowed to add any other members to the same which they think may be necessary to further the objects of the appointment.

On motion of Drs. MENDENHALL and ATLEE, it was ordered that the names of C. E. Cleaveland and James B. McClintock should be expunged from the list of permanent members of the association.

Dr. STOCKER of Pennsylvania offered the following amendments to the constitution, which laid over according to the rule:

Article 3 to read the association shall meet biennially in the city of ———. The place of meeting for the intermediate year shall be determined by a vote of the association.

Amend article 4 by providing for one permanent and two assistant secretaries, and also specifying the duties, &c. of each.

Dr. DORSEY of Ohio offered the following:

Resolved, that in May 1858, and every third year thereafter, this association

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meet at Washington city, and that the present officers be requested to correspond with the board of managers of the Smithsonian institute in regard to furnishing necessary rooms for the keeping of the archives of the association.

This resolution was laid on the table; but, subsequently, a special committee was appointed to confer with the regents of the Smithsonian institution on the subject.

On motion, the physicians of Canada were invited to attend the meeting at Nashville next year.

Dr. PHELPS, of the committee on the eminent and worthy dead, reported resolutions relative to the death of Dr. Gooch, a copy of which we failed to obtain.

Dr. SHEETS of Ohio offered the following, which was tabled:

Resolved, that it is derogatory to the dignity of the medical profession to notice the works of irregular practitioners in our medical periodicals.

The reports of Drs. Breckenridge and Gross on medical literature were then reconsidered, and their antagonistic views were discussed at some length. They were finally referred to the committee on publication.

Dr. PALMER reported in favor of granting leave to Dr. Hamilton to make use of his report on deformities after fracture, in his forthcoming work.

Dr. DAVIS, chairman of committee on the changes in the composition and properties of the milk of the human female produced by menstruation and pregnancy, presented a report; which was referred to the committee on publication.

Dr. NEWMAN of Buffalo, New York, chairman of committee on the sanitary police of cities, presented an elaborate report; which was referred to the committee on publication.

Afternoon Session.

Dr. PEREGRINE WROTH of Maryland submitted a partial report on the medical topography of the eastern portion of Maryland; which was put on file.

Dr. FULLER of Maine made a report on the best treatment of cholera infantum, which stated that the pathology of the disease was little understood, and recommended that physicians should freely interchange opinions on the subject.

Dr. GREEN of New York read a report on the use and effects of application of nitrate of silver to the throat, in which he asserted that great benefits had been derived from topical medication in tuberculosis, bronchitis, &c. Referred to committee on publication.

Dr. FLINT of Kentucky, chairman of committee on best mode of rendering the medical patronage of the national government tributary to the honor and improvement of the profession, made a report. He denounced the granting of patents by the United States government to quack medicines; but stating, however, that it appears from a letter written by the present commissioner of patents, that the practice of the office has been to discourage such a use of its functions, and that during the past fifteen years but four or five such patents have been granted; though from twenty to thirty applications per year had been made therefor. The credit of sanitary improvements, the doctor said, was not due to individuals, but to medical science.

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The committee on nominations then reported the standing and special committees for the ensuing year, which were approved:

Committee on Publication—Drs. Francis G. Smith of Pennsylvania, (chairman,) Casper Wistar of Pennsylvania, Samuel L. Hollingsworth of Pennsylvania, Samuel Lewis of Pennsylvania, H. F. Askew of Delaware, William Brodie of Michigan, R. C. Foster of Tennessee.

Committee on Prize Essays—Drs. Wm. K. Bowling of Tennessee, (chairman,) E. B. Haskins of Tennessee, Thomas Lipscomb of Tennessee, A. H. Buchanan of Tennessee, B. W. Avent of Tennessee, W. A. Cheatham of Tennessee, Paul F. Eve of Tennessee.

Committee of Arrangements—Drs. C. K. Winston of Tennessee, (chairman,) Ira Conwell of Tennessee, William D. Haggart of Tennessee, J. L. C. Johnson of Tennessee, F. A. Ramsay of Tennessee, George Grant of Tennessee, J. B. Lindsley of Tennessee.

On Medical Education—Drs. E. Geddings of South Carolina, (chairman,) C. W. Le Boutillier of Minnesota, G. F. Mitchell of Ohio, S. W. Clanton of Alabama, S. W. Butler of New Jersey.

On Medical Literature—Drs. R. Hills of Ohio, (chairman,) D. W. Yandell of Kentucky, R. R. Porter of Delaware, H. A. Johnson of Illinois, Charles E. Swan of Maine.

Special Committees.

Dr. Peaslee of Maine, on inflammation, its pathology, and its relations with ulceration.

Drs. J. C. Hutchinson of Brooklyn, New York, and Charles E. Isaacs of New York city, on the anatomy and histology of the cervix uteri.

Dr. J. Taylor Bradford of Augusta, Kentucky, on the treatment of cholera.

Dr. Mark Stephenson of New York, on the treatment best adapted to each variety of cataract, with the method of operation, place of election, time, age, &c.

Dr. J. W. Corson of New York, on the causes of the impulse of the heart, and the agencies which influence it in health and disease.

Dr. D. Meredith Reese of New York, on the causes of infant mortality in large cities, the source of its increase, and the means for its diminution

Dr. J. Foster Jenkins of Yonkers, New York, on spontaneous umbilical hemorrhage of the newly born.

Dr. Henry Carpenter of Lancaster, Pennsylvania, on the use of instruments in obstetrical practice.

Dr. Alex. J. Semnes of Washington, D. C. on the measures to be adopted to remedy the evils existing in the present mode of holding coroners' inquests.

Dr. J. Marion Sims of New York city, on the treatment of the results of obstructed labor.

Dr. J. B. Flint of Louisville, Kentucky, on the true position and value of operative surgery as a therapeutic agent.

Dr. G. Volney Dorsey of Piqua, Ohio, on the causes and cure of indigestion, especially in relation to the therapeutic indications to be derived from the chemical composition of the deposits of the urine.

Dr. C. B. Coventry of Utica, New York, on the medical jurisprudence of insanity, and the testimony of skilled witnesses in courts of justice.

Dr. Jos. Leidy of Philadelphia, Pennsylvania, on human, animal and vegetable parasites.

Dr. M. D. Darnall of Bainbridge, Indiana, on the value of a strict attention to position in the treatment of diseases of the abdomen.

Dr. George Sutton of Aurora, Indiana, on milk sickness.

Dr. Clark J. Pease of Janesville, Wisconsin, on the blending and conversion of the types of fever.

Dr. B. S. Woodsworth of Fort Wayne, Indiana, on the best substitute for cinchona and its preparations in the treatment of intermittent fever and malarious neuralgia.

Dr. Franklin Hinkle of Marietta, Pennsylvania, on the use of cinchona in malarious diseases.

Henry V. Campbell of Augusta, Georgia, on the nervous system in febrile diseases.

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Dr. John Neill of Philadelphia, Pennsylvania, on the laws governing the deposit of bone.

Dr. John W. Greene of New York city, on the intimate effects of certain toxicological agents in the animal tissues and fluids.

Dr. George Finley, U. S. A. on the medical topography and fauna of Washington territory.

Dr. James Cooper of Hoboken, New Jersey, on the flora of Washington and Oregon territories.

Dr. Charles E. Isaacs of New York, on the intimate structure and the pathology of the kidneys.

Dr. Israel Moses of New York city, on the diseases incidental to Europeans from temperate climates in their transition through Central America.

Dr. T. W. Gordon of Georgetown, Brown county, Ohio, on the etiology and pathology of epidemic cholera, to be continued three years, and with power to add any other members.

Dr. H. A. Johnson of Chicago, on the excretions as an index to the organic changes going on in the system.

Dr. D. D. Thomson of Louisville, on the remedial effects of chloroform.

The following physicians were appointed to fill vacancies in the committee on epidemics:

Dr. Fitz of New Hampshire, and Dr. Murray, U. S. A. for California.

Vacancies in the committee on a uniform system of registration were filled as follows: Dr. Woodward of Castleton, Vermont; Dr. Casey of Middletown, Connecticut; Dr. R. W. Haxall of Richmond, Virginia; Dr. A. Stout of San Francisco, California.

[We conclude that the committee on epidemics for Virginia and North Carolina has been abandoned as a hopeless undertaking.]

Dr. D. S. CAIN's report on the epidemic yellow fever of Charleston was received and referred.

An abstract was read of Dr. FENNER's report on the yellow fever of New Orleans; the report itself was referred to the publishing committee.

Dr. GUNN, from the committee on arrangements, reported that he had three voluntary communications from Dr. Isador Gluck of New York, on the use of sulphate of cinchona in periodic diseases; Dr. G. P. Hackenberg of Springfield, Ohio, on the formation of gunshot wounds in general and especially on the head, with remarks on the effect of gunshot wounds on the brain and its functions; Dr. Charles R. Chandler of Rochester, Boone county, Missouri, on an improved method of applying compression to the scrotum, requesting that they may be referred to a special committee, as they have not the time possibly to examine them preparatory to referring them to the convention and the committee on publication, or rejecting them.

Dr. WATSON of New York hoped that the committee on arrangements would not be excused. There was complaint made last year by Dr. Corson of New York, that last year's committee had treated a paper which he then sent in with disrespect, and refused its admission into the Transactions. He complained bitterly of his treatment in the matter.

Dr. DAVIS of Illinois, chairman of the last year's committee, replied, stating that Dr. Corson had no reason to complain, and that his allegations were unfounded. The committee who conferred with Dr. C. had assured him that he must revise his paper, and he had expressed a willingness to do so. The Dr. never had done this, and his paper had consequently never appeared.

The communications were referred to a special committee, consisting of Drs.

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Stille of Philadelphia, S. Jackson of Northumberland, Pennsylvania, and J. R. Biddle of Philadelphia.

Dr. DORSEY of Ohio offered a resolution that the committee on the etiology and pathology of cholera be instructed to memorialize the congress of the United States, requesting that honorable body to grant every necessary assistance which can or will promote the objects for which the committee has been appointed. Carried.

Dr. WISTAR of Pennsylvania offered a resolution that a committee of three be appointed to correspond with the proper officer of the Smithsonian institute, enquiring into the possibility of procuring a chamber in that institution for the use of the association. Carried, and Drs. Wistar of Pennsylvania, Hale of Washington, and J. Neill of Pennsylvania, appointed such committee.

A member of the committee on registration reported that the committee could not report fully on account of the death of the chairman, Dr. Wilson of Connecticut; a partial report was submitted and filed.

Dr. NEILL of Philadelphia offered a resolution that no medical preparation, account of surgical operation, or anything else designed or calculated to give notoriety to an individual, be laid before the association, until reported upon by a special committee.

This was regarded as a personality by Dr. Wood of New York, the member who had presented a pathological specimen for the instruction of the association. He mounted the platform, and retorted vigorously, and with an indignation that was not unbecoming. He denied that he sought notoriety. He observed that lucifer match makers' disease was an obscure subject on which he desired to throw light.

After a pleasant *bon mot* by Dr. THOMSON of Delaware, about removing the bone of contention and holding their jaw, peace was restored; the motion for the presentation of the extirpated maxilla was stricken from the minutes, and Dr. NEILL withdrew his resolution.

Dr. DORSEY of Ohio offered the following resolution, which was adopted:

Resolved by the American medical association, that the committee on the etiology and pathology of cholera be instructed to memorialize the congress of the United States, requesting that honorable body to grant every necessary assistance which can or will promote the objects for which the committee has been appointed.

The SECRETARY announced a letter from the Royal medical and chirurgical society of England, acknowledging the receipt of the eighth volume of the Transactions of this body.

Dr. PHELPS of New York offered the following resolutions, which were adopted:

Resolved, that the thanks of this association are due, and are hereby tendered to the fire department of the city of Detroit, for the gratuitous use of their large and commodious hall, so amply furnishing to us accommodation for the convenient transaction of business.

Resolved, that the urbane deportment and elegant hospitalities of the profession and of private individuals, as well as the polite attentions of citizens generally, demand of this association a high appreciation of the cultivated manners of this city of the west, and which has tended greatly to enhance the pleasure of the session here of the delegates from abroad.

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Fourth Day, May 9th, 1856.

The association met at nine o'clock, but as many members had left for their homes, the attendance was small.

Dr. PALMER moved, by request, that Dr. Coolidge, U. S. A. should be appointed a member of the committee on epidemics, in place of Dr. Finley, U. S. A. which was agreed to.

Dr. LINDSLEY reported resolutions of respect for the memory of P. Claiborne Gooch, which were placed on the minutes.

Dr. DENTON of Michigan offered the following, which was rejected:

Resolved, that a committee of three shall be appointed, whose duty it shall be to enlist some enterprising publisher to aid in collecting and arranging material for an American medical directory.

Dr. LEIDY of Pennsylvania offered the following:

Whereas, it is the object of this association, in the award of prizes for communications on subjects appertaining to medical science, to encourage the progress of the latter, and as this result cannot be better attained than through original investigations and discovery,

Resolved, that hereafter an annual prize of $—— be awarded for the best memoir or essay founded on original investigations of the author, and in case of no memoir or essay being presented worthy of such award, the prize money to be appropriated towards the expenses of publishing and illustrating such memoirs as may be subsequently deemed worthy of an award. This with the suggestions of the committee on prize essays was referred to a special committee consisting of Drs. Leidy, G. B. Wood and C. D. Meigs, all of Philadelphia.

Dr. ATLEE of Pennsylvania offered a resolution that hereafter all voluntary communications be referred to a special committee, to be appointed by the president on the first day of each annual session, who shall examine and reject the same or refer them to the committee on publication. Adopted.

On motion, it was

Resolved, that the committee on publication be requested to transmit annually to the Epidemiological society of London a copy of our Transactions.

On motion of Dr. GUNN of Michigan,

Resolved, that any new medical institution not heretofore represented in this body be requested to transmit to the secretary, with the credentials of its delegates, evidence of its existence, capacity and good standing. Carried.

On motion, the resolutions appended to Dr. Palmer's report on the plan for organizing state and county societies, were taken from the table and referred to the committee on publication.

Dr. PHELPS of New York offered a long series of resolutions, with a voluminous preamble, all of which were laid on the table.

After resolutions of thanks to the different bodies and individuals at Detroit, from whom the members had received courtesy, the association adjourned to meet at Nashville on the first Tuesday in May 1857.*

* Owing to unavoidable engagements, neither of the editors of this journal was able to attend the meeting at Detroit. They have prepared the above report of the proceedings from minutes taken by a correspondent, and from numbers of the *Detroit Free Press*, kindly forwarded by him. They presume it will be found correct in all essential particulars.

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VARIETIES.

Medical News and Items.

VACCINE TROUBLES.—In Dr. Ditterich's Neue Medicinisch-Chirurgische Zeitung, for June 1854, an account is given of an occurrence at Bamberg, which is said to have excited an immense sensation throughout Germany at the time. A physician was accused of inoculating with syphilis children vaccinated by him, and was tried and condemned. The details of this affair are briefly as follows: In the summer of 1853, Dr. Hubner, a respectable physician, who, for twenty years, had practiced all the vaccinations in a large district in Franconia, vaccinated a number of children with virus taken from the arm of a child whose face and body were covered with an eruption. After a longer or shorter period, eight of these children manifested symptoms of constitutional syphilis. The witnesses stated that no legitimate vaccinal pustules had been developed, but little purulent vesicles, which soon became ulcers. These children communicated the disease to nine adults. The true nature of the affection was not detected until the appearance of ulcers, condylomata, etc. A remarkable circumstance, which would seem to exculpate Dr. Hubner, was that other children vaccinated with this same matter had displayed no abnormal symptoms. An inquest discovered that the mother of the child with the suspicious eruption had once had syphilis, though she protested that she was not infected during the gestation of this infant. Dr. Hubner was tried before a jury chiefly composed of physicians, and was condemned to imprisonment for one year. He appealed to a second tribunal, which extended the term of confinement to two years. Finally, a third court being appealed to, Dr. Hubner was sentenced to six weeks' imprisonment and costs. The German journals regard this case as a clear instance of inoculation of secondary syphilis, a question which has been so much debated. As to the mode in which the syphilitic virus was transmitted, numerous hypotheses are offered. It is supposed that matter from the suspicious vesicles united with the vaccine virus, or that the blood of the child mixed with it, or that the vaccine matter itself was infected. Dr. Hubner was generally considered blameworthy, inasmuch as it was imprudent to use the vaccine matter from the child with the eruption, even if this eruption was not apparently syphilitic in its nature. The punishment was considered severe; that of the second court unjustly so.

ALEXIS ST. MARTIN.—The subject of Beaumont's remarkable experiments is now in New York, in good health. It is hoped that he will be induced to allow himself to be the instrument of farther research.

DR. WARREN'S WILL.—Peremptory provisions of his will required that the following disposition should be made of Dr. Warren's body. Twenty-four hours after death the veins were to be injected with arsenical solution; at the end of the next twenty-four hours, the funeral ceremony should take place, and the body be deposited under St. Paul's church, Boston; and twenty-four hours there-

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after, the body was to be given to the officers of the Massachusetts medical college, for examination; after which the flesh was to be removed, the skeleton macerated, bleached, wired, and deposited in the college museum. These directions have been complied with, as far as time will permit. At the post-mortem examination, no organic disease was discovered. The testamentary directions above detailed, which appear, at the first glance, ostentatious, eccentric, and in bad taste, are explained and justified by Boston physicians, on the ground that they were prompted by the desire of the deceased to do away with the morbid sensibility with which the public regards anatomical pursuits.

SPHYGMOMETER.—Dr. Vierodt, the famous German physiologist, struck by the advantages of the hæmadynamometer in physiological research, has invented an instrument designed to measure arterial pulsations after a different fashion, which he calls a *sphygmometer* (*sphygmos*, the pulse.) A modification of this instrument, the *kymograph*, gives a graphic representation of the arterial waves. We cannot describe this complicated apparatus. Suffice it to say that a transverse bar, which presses on the artery, is connected with a hair pencil, which marks the pulsatory undulations on a paper blackened with lamp-black. The lines thus formed are varnished over and preserved. We compile this brief notice from an account of the invention in Guerin's Gazette.

HOLMES' EULOGY OF WARREN.—*Mr. President*—Death has just removed from our midst one long known to us as a leading member of our various local associations; to this community as a most valued professional counseler and honored citizen; to the profession itself as a master in one of its leading departments, and a laborious teacher of more than a whole generation of practitioners; to the country as one of its ornaments, and to men of learning every where as a liberal and enlightened student of nature. The name of John Collins Warren is stricken from the roll of living men.

There is no man here, whatever his age or standing, that can bear this brief announcement unmoved. To the old it is a sudden breaking up of associations that half a century of active life has been slowly knitting together. To the young it is one of those startling changes that shift the whole vista of the future; life slides forward a whole stage when those who stand in full relief upon its furthest confines drop beneath the horizon. We have all grown older in more than days since yesterday; we have lost a presence that filled no small space in our habitual outlook, and passed it over to the ever widening domain of memory.

There have been few men in the time of the oldest among us who have stamped their character more distinctly on their associates than he whom we must now speak of as belonging to the past. He entered life with singular advantages. His father was the leading surgeon of the leading town of New England; had served his country faithfully in the camp and on the field; had founded a school, and was known as an eloquent and ardent teacher. His uncle had shed imperishable lustre upon the name he bore; his alliances gave him influence; his career was unimpeded by the embarrassments common to many who rise in spite of them to eminence.

It is not much only to inherit advantages, as every day shows us but too

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clearly; we see the new men carrying off the prizes in every calling, in the face of the hereditary occupants of power and position. But it is much to know how to bear the temptations of good fortune, or what is so called; to cast off indolence, to despise self-indulgence, to work from a high sense of duty, or even from a noble ambition, as others work from hard necessity.

Whatever place Dr. Warren acquired or maintained in life, no man can say that he did not earn it and keep it by his own fair labor. In his great centre of life, where an overworking race sends its strongest muscles and its busiest brain to be worn out, it would be hard to name the man who toiled more unremittingly than he, during the busier years of his life. In his vast practice at the hospital, of which he was one of the founders, and where he passed so large a share of his time; in the professor's chair, the offices of which he performed with signal fidelity and punctuality; every where, he was unsparing of his time and his labor. Those of us who met him at that busy period of his life, remember him as grave, concentrated, often stern, a man of few words, and those apt to be peremptory; one who went his way bent on his own task, and not likely to be turned aside from it.

But neither all the advantages he inherited, nor all the toil he expended, could have given him the place he attained, without elements of personal superiority to lend vitality to both. Somewhere in the mind, or in the character, or in both, must be found the source of that remarkable influence which Dr. Warren exerted during a long series of years, amidst all the competition and the changes of city professional life. If we should look only at his purely intellectual qualities, we should not have reached the secret of his mastery. The varied intellectual power, the wide range of knowledge which belongs to the scholar who lives in the world of thought, are not to be expected in the men whose lives are passed in the practical use of applied science. From them we can only demand accuracy instead of breadth of view, sagacity instead of erudition, readiness in the place of versatility. These are the qualities that must belong to the successful surgeon, and these, with a practiced hand and unshaken nerves, were generally granted by the profession and the public to belong to Dr. Warren. But to these qualities, which fitted him for superiority in his peculiar department, were added two other traits, which lay underneath all the rest, and gave them their consummate effectiveness: unswerving concentration of purpose and unbending force of will. These gave him his unchallenged supremacy in the professional sphere he had chosen.

To understand his character, we must compare that busy period of his life before referred to with its later years, after he had relinquished the most arduous portion of his daily duties. Then it was that the taste for natural science, held sternly in abeyance during a long period of professional toil, was allowed to assert itself, and all might see how resolute must have been the purpose that could have kept it subjugated, and almost unsuspected. Then it was that the pleasant social qualities, overlaid for a time by the weight of severe occupation, found their spontaneous expression—and all could feel that the somewhat austere aspect of his overtasked middle age was only another proof that he had given his whole mind, and heart and strength to cares that might subdue his natural vivacity and sadden his cordial smile.

These last years of his life have softened all our recollections of his strenuous

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years of toil. He had got out of the brawling current, and as he neared the farther shore, a quiet eddy carried him far back towards the fountains of his youth. A kindly old man, full of pleasant anecdote, busy with ingenious speculation, loving nature always, and studying her, not as once, in the fearful shapes in which she used to challenge his skill, but under the branches of the "Great Elm," or beneath the buttressed ribs of his huge Mastodon, or hanging over the sandstone tablets where the life of the eternity that is past has left its earlier autograph, he pursued his cheerful labors to the last, bent, but not broken, and so walked softly from among us into the land of shadows.

THE ACCOUCHEMENT OF THE FRENCH EMPRESS.—All the world knows that the long expected heir of the Napoleonic dynasty has at last safely arrived, and France rejoices over the birth of the king of Algiers.

The nephew of "*mon oncle*" has in this transaction exerted his powers of imitation to an incredible degree, and almost succeeded in forcing nature herself to acknowledge his sway. The empress Eugenie was brought to bed within three days of the anniversary of the birth of Maria Louisa's son. Antoine Dubois, the great obstetrician, who safely delivered Napoleon's empress of the "king of Rome," was no more. So Paul his son reigned in his stead. The king of Rome was delivered after great difficulties; a shoulder presentation having been, by cephalic version, changed into a natural labor. The king of Algiers, scorning to be eclipsed by his predecessor, was delivered with forceps.

'Tis said that chloroform was administered to Eugenie, but its effects were so unpleasant that its use was discontinued. The dexterity of Paul Dubois was not enough to prevent the formation of a slight bruise upon the cheek of the "*enfant de France*," but it has happily passed away, and we are glad to know that her majesty, having safely passed through her milk fever, is "doing as well as can be expected."

To complete the parallel, both Antoine Dubois and his illustrious son were rewarded with the grand cross of the legion of honor, and the title of baron; but much to the surprise of the public, Paul Dubois has only received *six thousand dollars* for his forceps case, whilst old Antoine made a cool *twenty thousand dollars* out of a shoulder presentation.

Does Louis Napoleon think an instrumental delivery worth less than cephalic version?

THE STABLE AND THE TABLE.—That the horse is capable of supporting man is a fact which has long been recognized by fox hunters and most others, but certain persons in France are now asserting it in a sense entirely new. They are endeavoring to prove, both by theory and practice, that the horse is similarly capable of supporting man with the pig. M. Geoffroy de St. Hilaire, the chemist, is the chief of these hippophagi, or horse eaters, who, having divested themselves of unphilosophical prejudice in favor of beef and mutton, prefer another kind of meat instead.

That horse-flesh would probably go faster than any other is a consideration which may present itself to a superficial mind; but that such is not the case may be known to many persons who may have ventured on a plate of hashed venison at a cheap eating-house, and have experienced an amount of difficulty in getting

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through it sufficient to prove that horse-flesh would go very slowly, although it might possess the economical advantage of going very far.

It is not unlikely that the flesh of the thorough bred horse would be characterized by a peculiar raciness of flavor, less observable in the colt than in the full grown animal, entered some time upon the turf—whereon horses, if destined to be eaten, would be entered for cups in a minor proportion than for plates, and not only for plates, but also for knives and forks and dishes.

Newmarket would supply a happily named locality for the erection of shambles for the sale of horse; though it may not follow that the best coursers would necessarily furnish the best material for one of three courses; and Dobbin, in a gastronomical point of view, may be preferable to Eclipse.

Should the use of horse-flesh obtain in this country, attention would of course be directed to the improvement of the breed in a novel direction.

The dray horse, greatly exaggerated, would accompany the Hereford ox at the fat cattle show; and the hunter and cob would be intermingled with Devons and short horns, exhibiting flanks of enormous latitude, and huge mountains of fat on their withers.

How pony steaks would taste, what kind of thing would be a round of nag, a sirloin, or ribs, or breast of filly; whether horse would require horse radish, may be matters of speculation; but in England, at least, will be matters of speculation only whilst the living horse fetches a price so much higher than would be given for his mere carcass. Eating horse would be eating money indeed; and the slaughter of an animal worth perhaps three thousand pounds would be an extravagance too prodigious for any table, inclusive of the civic; it is not therefore to be expected that any mayor will ever eat horse.

The "high mettled racer," instead of being sent to the bounds, might certainly be consigned to M. de St. Hilaire and his disciples; but as it is impossible to have your horse and eat him too, most persons would prefer the possession to the meal, if the horse were good for any thing, and few would be disposed to regale themselves on a good for nothing horse.

There may be a singular and extraordinary daintiness in the chief of the equine family, of which its inferior members do not partake; otherwise, why confine horse eating to the equus caballas?

There is the equus asinus also—would not that humbler and cheaper individual of the genus content the hankerers for horse? Even if there were the same relation between the noble and the inflexible animal as that of the rabbit to the hare, they might dine very well upon donkey. But their nature instinctively revolts, perhaps, at a banquet which, to them, may seem what that of a Carib appears to civilized people.

On the whole, it is probable that except by the dogs, and the crows, and the consumers of doubtful sausages, the horse will remain untouched as an article of food, and nothing be eaten more nearly related to it than the saddle—of mutton.—*London Punch.*

Specific for black vomit.—In the beginning of 1853, her majesty's vice consul at Bolivar, Venezuela, becoming afflicted with the cacoethes scribendi, inundated the several West India governments and colonial newspapers with accounts of the discovery made by a Madame Orfila of a certain, sovereign and infallible cure for black vomit, effected by a plant indiscriminately named verbena

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and vervena, which is very abundant. And of this plant a specimen leaf was always forwarded, which, like the brick, displayed as a specimen of a house, in the old joke of Theophrastus, was intended to convey full and complete information of the plant. There were, however, often two leaves sent, one larger than the other. The one was stated to belong to the male, and the other to the female verbena. The name of Orfila, although in this case it designated an ignorant old half-blood Indian woman, no doubt promoted the renown of this new remedy.

One of the worthy consul's circulars, of course, reached Guiana, and his excellency the governor, as it came in official garb into his hands, very properly referred it to the surgeon general, with instruction for a trial of the remedy, and a report of results to be forwarded to the government secretary's office. The statements of the circulars contained abundant evidence of scientific ignorance, and were replete with absurdities. But still it was possible that a savage or an ignorant person might stumble on a great medical discovery—the cinchona, for example—and be unlucky in the expounder. I therefore set about obtaining information which would lead to a knowledge of the plant really meant. As the genus verbena is pretty extensive, including even East India teak in its family, it was not so very easy to determine the species and variety so highly recommended.

At length, having obtained an entire plant of what was admitted by those, who should know, to afford the genuine remedy, I discovered, through Mr. W. H. Campbell, whose name is a sufficient guarantee for its accuracy, that this treasure was the *stachytarpha Jamaicensis*. The nauseous and disgusting compound was prepared and administered precisely according to directions, and, it need scarcely be added unsuccessfully. Its want of success, however, was less a matter of concern, as about this time the worthy vice-consul, ever anxious, as he declared himself, for the welfare of mankind, announced through the newspapers the discovery of another sovereign and infallible remedy for the same complaint.

It requires apology for referring to this ridiculous affair, but the verbena of Madame Orfila has been, I perceive, the subject of grave conversation in the London epidemiological society.—*Baker's Report on Yellow Fever.*

Reminiscences of Doctor James Craik, the physician and friend of WASHINGTON.—Dr. Craik was born in Scotland, where he received his education for the medical service of the British army. He came to the colony of Virginia in early life, and had the honor to accompany the youthful Washington in his expedition against the French and Indians in 1754, and returned in safety after the battle of the Meadows and surrender of Fort Necessity. In 1755 he attended Braddock in his march through the wilderness, and, on the 9th of July, assisted in dressing the wounds of that brave, but unfortunate commander. At the close of the French war, the subject of this article resumed and continued his professional labors till the commencement of the revolution in 1775.

By the aid of his early and fast friend, General Washington, he was transferred to the medical department in the continental army, and rose to the first rank and distinction.

In 1777 he had an opportunity, which he gladly embraced, to show his fidelity to his general and to his adopted country, by taking an active part in the develop-

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ment of a nefarious conspiracy, the object of which was the removal of the commander in chief.

In 1780 he was deputed to visit Count de Rochambeau, then recently arrived at Rhode Island, and to make arrangements for the establishment of hospitals to accommodate the French army. Having performed this difficult duty, he continued in the army to the end of the war, and was present at the surrender of Cornwallis, on the memorable 19th of October 1781.*

After the cessation of hostilities, the doctor settled as a physician in Charles county in Maryland; but soon removed to the neighborhood of his illustrious friend and companion, the farmer of Mount Vernon, at his particular, repeated and urgent request. In 1798, when like a guardian angel, the never to be forgotten Washington again stepped forth to redress the wrongs of his country, the venerable Craik was once more appointed to his former station in the medical staff. With the disbandment of the army then called into service, ceased the public professional labors of the subject of this memoir, whose life, for nearly half a century, had been devoted with zeal and high reputation to the cause of his country.

One trying duty yet remained to be performed. It was to witness the closing scene, and to receive the last sigh of his revered commander, the most distinguished man of his age. Their youthful commissions had been signed on the same day. They had served together in the ranks of war. Their friendship was cemented by a social intercourse of fifty years' continuance, and they were greatly endeared to each other by common toils, privations and honors. At length the moment of parting arrived. It was tender, affectionate, solemn and impressive. In reference to that painful event, the doctor is said to have expressed himself in this manner: "I, who was bred amid scenes of human calamity, who had so often witnessed death in its direst and most awful forms, believed that its terrors were too familiar to my eye to shake my fortitude; but when I saw this great man die, it seemed as if the bonds of my nature were rent asunder, and that the pillar of my country's happiness had fallen to the ground."

As a physician, Dr. Craik was greatly distinguished by his skill and success, and his professional merits were highly and justly appreciated. In the various relations of private life, his character was truly estimable, and his memory is precious to all who had the happiness and the honor of his acquaintance. He was one (and what a proud eulogy it is) of whom the immortal Washington was pleased to write, "*my compatriot in arms, my old and intimate friend.*" He departed this life at the place of his residence in Fairfax county, on the 6th of February 1814, in the 84th year of his age.—*Thatcher's Am. Med. Biography.*

THE MEDICAL PROPERTIES OF RUE.—Rue, called by the ancient Greeks rutæ, was first noticed by physicians because of its strong odor and extreme bitterness, and was often employed as an antidote to the poison of hemlock, and as a tonic in certain affections of the eyes. Ovid has alluded to it in his remedium amoris, and it is to it he applies the verse, "Acuentes lumina rutas."

It is remarkable that from the remotest time down to the present day, the

* Dr. Craik was the director general of the hospital at Yorktown, and the present author has a distinct recollection of receiving from his hands the necessary surgical implements when a battle was expe ted on our approach to the British lines, and of receiving his directions to keep near in the view of the Marquis De Lafayette, and to pay the first attention to him in case he should be wounded.

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Greeks have always applied little poultices of the powdered rue to inflamed eyes. They use also an oil of rue prepared by macerating the plant in olive oil. The Turks also use the essential oil of rue.

There is another of the rutacæ much used in Greece. It is the peganum harmala, the peganon of Dioscorides. Its properties appear to be narcotic, and resemble those of the cannabis indica. The Greeks and Turks use it after maceration, and also as an infusion in water. This plant is frequently ordered as a cataplasm in cramps of the belly.

LINES ADDRESSED TO A SKULL.—The following beautiful verses were found in the skeleton case of St. Bartholomew's hospital more than thirty years ago, placed there by some unknown hand. Breathing alike the true spirit of poetry and religion, we take pleasure in perpetuating this anonymous production:

Behold this ruin! twas a skull,
Once of æthereal spirit full;
This narrow cell was life's retreat,
This space was thought's mysterious seat.
What beauteous pictures filled this spot!
What dreams of pleasure—long forgot!
Nor love, nor joy, nor hope, nor fear,
Has left one trace or record here.

Beneath this mould'ring canopy
Once shone the bright and busy eye;
Yet start not at the dismal void!
If social love that eye employed;
If with no lawless fire it gleamed,
But with the dew of kindness beamed,—
The eye shall be for ever bright,
When stars and suns have lost their light.

Here, in this silent cavern, hung
The ready, swift and tuneful tongue;
If falsehood's honey it disdained,
And where it could not praise, was chained;
If bold in virtue's cause it spoke,
And gentle concord never broke;
That tuneful tongue shall plead for thee
When death unveils eternity.

Say! did these fingers delve the mine?
Or with its envied rubies shine?
To hew the rock or wear the gem,
Can nothing now avail to them;
But if the page of truth they sought,
And comfort to the mourner brought;
These hands a richer meed shall claim,
Than all that waits on wealth or fame.

Avails it whether bare or shod,
These feet the path of duty trod?
If from the bowers of mirth they fled,
To soothe (?) affliction's humble bed,
If grandeur's guilty bribe they spurned,
And home to virtue's lap returned;
These feet with angel's wings shall vie,
And tread the palace of the sky!

LETTER FROM HUMBOLDT.—Some persons reported the author of Cosmos to be a believer in the mysteries of table moving, because he had approved of the theory of the electricity of the nerves, started by Du Boys Reymonds. Dr. Jobard thereupon wrote to Humboldt, to ascertain his opinion about spiritualism, and received the following answer:

"*Berlin*, April 2d, 1856.—You have written to me, my dear sir, as you always do, a most amiable letter, but I am not able to give you my opinion upon the possibility of the existence of the various kinds of mineral, vegetable, animal, direct or indirect cerebral electricity. I have a holy horror of all kinds of pine wood spiritualism and psychographic mysticisms. You increase my horror by the ghost of that ephemeric being of reason, which is to receive intelligence from the thoughts of those persons who surround the table. You know that Geoffrey Saint Hilaire pretends to have sweat the oxyde of thought while in Egypt, and you, my dear friend, will say, that my incredulity is the simple consequence of my laziness. I submit willingly to this reproach, for I am convinced that the friendship, which you always extended to me, will not be diminished if I can be instrumental in leading you out of the obscure path of error. I rely on your forgiveness.

A. V. HUMBOLDT."

[*Boston Med. & Surg. Journal.*

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Legal Responsibility.—Judge Minot of Pennsylvania, has laid down the following rules of law, as applicable to physicians: 1. The medical man engages that he possesses a reasonable degree of skill, such as is ordinarily possessed by a profession generally. 2. He engages to exercise that skill, with reasonable care and diligence. 3. He engages to exercise his best judgment, *but is not responsible for a mistake of judgment.* Beyond this, the defendant is not responsible. The patient himself must be responsible for all else; if he desires the highest degree of skill and care, he must secure it himself. 4. It is a rule of law that a medical practitioner never insures the result. These are received in general as sound views, and such as will govern every enlightened court. There could scarcely be a greater absurdity, than to require physicians and surgeons to insure the result, when they can in no case control all parts of the treatment. Few serious cases are carried through a single day, and many not a single hour, without a violation of instructions, on the part of nurses and attendants.—*Peninsular Journal of Medicine and Collateral Sciences.*

New Test for Nux Vomica.—The following test, proposed by Vielgruth, and approved by Wittstein, for detecting nux vomica, recommends itself for its simplicity:—A few grains of the substance supposed to contain nux vomica is treated with proof spirit. The tincture thus obtained is evaporated to dryness on a water bath, at a temperature of about 95 degrees. To the residue, a drop or two of dilute sulphuric acid is added. The whole is again exposed to the above mentioned temperature, when, if nux vomica is present, a beautiful carmine red color ensues. If the heat is stopped in the course of ten or fifteen minutes, this color disappears; but it will reappear, although of diminished brightness, by a renewal of the heating. If the temperature be increased, the color becomes reddish brown, and finally black, in consequence of the separation of carbonaceous particles.—*London Lancet.*

NECROLOGY.

Warren.—Dr. John Collins Warren, the eminent Boston surgeon, died on the 4th of May, in the 79th year of his age. His death could not be attributed to any distinct disease; his health has been bad for several years, and his vital powers were finally exhausted, without any important local affection. Dr. Warren was born in Boston, August 1st, 1778. His father, Dr. John Warren, was a distinguished surgeon, and his uncle was the General Warren, the martyr of the revolution, whose name is so closely associated with the battle of Bunker Hill. Dr. Warren graduated at Harvard college in 1797. He pursued his professional studies in London, being a pupil of William Cooper, and subsequently of Astley Cooper. In 1806, Dr. Warren was appointed adjunct professor of anatomy and surgery at Harvard college, and in 1816, he succeeded his father in the full professorship. He held the chair of anatomy until 1847. He succeeded Dr. James Jackson as president of the Massachusetts medical society in 1832, and filled this office four years. In 1803, he married Susan Lowell Mason,

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by whom he had six children. She died in 1841. In 1843, he married Anna Winthrop, by whom he had no issue. She died in 1850. Dr. Warren was a friend of the temperance movement, and for a long time presided over the Massachusetts temperance society. At the organization of the Massachusetts general hospital in 1817, Dr. Warren was elected surgeon to this institution—a post he retained until a few years before his death. At the time of his death he was emeritus professor of anatomy and surgery in Harvard college, president of the Boston society of natural history, a member of the American academy of arts and sciences, of the American philosophical society, of the Philadelphia academy of natural sciences, of the academy of Naples, and the medical society of Florence, an honorary member of the medico-chirurgical society of London, and a corresponding member of the royal academy of medicine of Paris.

Percival.—Dr. James G. Percival, the gifted poet, died on the 2nd of May, at Hazelgreen, Wisconsin, in the sixty-first year of his age. Although it was long since he abandoned the practice of medicine, we cannot but remember with pride that he was a member of our profession, nor fail to express our respect for his memory. Percival was born at Kensington, Conn. in 1795. He was the second son of Dr. James Percival. He graduated at Yale college in 1815; reading as a commencement exercise his tragedy of "Zamor." In 1820, having graduated in medicine, he commenced the practice of his profession in Charleston, S. C. He did not succeed, however, and obtained, in 1824, an assistant surgeoncy in the army. He served for a while at West Point, and subsequently as examiner of recruits at Boston. But he soon left the service and his profession. He published two volumes of poems, which were reprinted in London in 1824. In 1827 he published a third volume. The next two years he spent in assisting in the first quarto edition of Webster's Dictionary. He then translated Malte-Brun's Geography. He was a good naturalist, and his attention became gradually more and more concentrated on geology. In 1842 he published an elaborate report on the geology of Connecticut. In 1853 he was chosen state geologist of Wisconsin, and at once went to work in that new and interesting field. He was a very superior man.

Gerdy.—P. N. Gerdy, professor of surgical pathology to the faculty of Paris, surgeon to La Charité, and member of the academy of medicine, died at Paris, April 18th, of pulmonary consumption, at the age of 59. The deceased was the author of several practical works, and engaged ardently in the discussions which have agitated the French for a quarter of a century past. He was reckoned the best debater in the academy. He was not much followed by foreign students, for his service at the hospital adjoined that of the great Velpeau. He was chiefly known in this country by his method for the cure of reducible hernia. In 1851 M. Gerdy commenced the publication of a treatise on surgery in seven monographs. He lived to complete four volumes of this work, comprising three monographs. The first treats of general pathology; the second of general surgical diseases; the third of diseases of the organs of locomotion. It was to complete this work that Gerdy remained in Paris, in opposition to the advice of his friends, during the winter of 1854-55, since which period the disease which long threatened him made rapid progress.

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